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Power and the Monopoly of Information

PHILIP E. CONVERSE
The University of Michigan

Presidential Address, American Political Science Association, 1984

The links between power and information are numerous and, for the most part, very obvious. I shall not apologize for returning to the obvious at many points in my remarks, on the grounds that at least a few things that are obvious are also deadly important and deserving of repetition from time to time.

It was about a generation ago that both constructs—power and information—became prize targets for more formal definition and measurement. Indeed, it is a charming coincidence that the publication of Orwell's novel *Nineteen Eighty-Four* (1949), dealing with power and information, coincided exactly with the watershed study by Shannon and Weaver (1949) on the measurement of information and the "mathematical theory of communication." The impact of such measurement has been enormous and continues to grow. Regrettably, however, this form of measurement will be of relatively little use to us here, because its bite is lost in open systems. That is to say, the lanterns in the steeple can convey to the knowledgeable one crucial bit of information—whether the British are coming by land or by sea—but they do not mean much unless the viewer is already apprised that this is a special communication device and knows in advance the full code of potential alternatives. We shall be operating in more open systems, where these helpful constraints are missing.

The numerous attempts from the same period to formalize the treatment of power will be no more helpful, even though most competing definitions of "the power of A over B" did have a similar ring about them, suggesting substantial convergence. We can, however, use one part of that literature to say what we are *not* talking about. Scholars have frequently differentiated among the various bases on which power may rest, and one base that customarily emerges on such lists is expertise. This is, of course, a classic

point of intersection between information and power, and one whose implications have yet to be fully catalogued. This will not, however, be my focus here, although the same phenomena will lurk in the background. Instead, my attention is upon the flow in the opposite direction: the use of power, such as the power of the state, to control information in ways designed to maintain and enhance power. Censorship is what we usually call it, in the small. Writ larger, we talk on the positive side of the "freedom of the press," and larger still in this country, of "First Amendment freedoms."

We cannot expect here to address more than a corner of this large and troubling subject.¹ Discussions of First Amendment freedoms typically come to center upon the expression of opinion and most naturally on those opinions that are unsettling to a regime because they stand in some form of opposition to its policies or premises. I hardly question the importance of such matters. But my primary interest here will be less upon suppression of opinion than upon the suppression of the flow of information about what is going on in the political world of observers both common and uncommon: in short, the suppression of news.

I do not pretend that it is easy to distinguish what I would like to discuss as facts and incontrovertible events from opinions and interpretations, because of course a wide sea of gray lies between these ideal types. "Facts" shade rapidly into "factoids," and on to powerful embroideries of judgment and construction. But shades of gray cloud most First Amendment issues, whether we are asking where pornography begins or "clear and present danger" ends.

So let us take a common-sense view of facts versus opinions. It is likely that government censors do. For example, a revealed copy of the censorship guidelines used 10 years ago by the Polish government makes this distinction routinely. Quite naturally, pure attitudes, in the form of praise of things the regime disliked or criticisms of things it liked, were forbidden, and numerous in-

At various stages I have profited from aid and suggestions from Kenneth A. Bollen, Ada Finifter, Fred Greenstein, Harold K. Jacobson, Seymour Martin Lipset, Mitchell A. Seligson, and William Zimmerman. Responsibility for flaws, however, rests with me.

¹A panoramic view, covering the myriad challenges posed by the flood of new information technologies, is provided by de Sola Pool (1983).

stances are mentioned in detail. But at least an equal share of the required deletions refer to what is called instead "information," to be expunged although its factual accuracy is not challenged. The list of banned topics here is lengthy, and I can only give a glimpse of it. To be obliterated were references to the sheer "existence" of many things, ranging from noted people to detailed forms of trade between the Polish government and other countries. News was not to be conveyed about the actual building or rebuilding of churches, nor was any information about "anti-Semitic excesses." Likewise any hint of the political rights and liberties enjoyed by Polish emigrants in other countries was to be deleted as well as mention of donations they had made back to Poland. Various data compilations were forbidden, such as those that would permit accurate deductions concerning relative international currency values. Also to be deleted was information about industrial accidents, relative rates of occupational disease, and especially information about what were called "direct hazards to human life through the use of chemicals in agriculture" and elsewhere (Pastecka, 1978).

In truth, it will not matter greatly that I prefer to move into the discussion from the side of fact because much of what I have to say can cover both fact and opinion. However, some of the wonderments I shall express remain more compelling where facts are at issue. Hence I shall assume, for example, that there really was a Khrushchev speech before the Twentieth Party Congress in 1956, even though it has yet to be reported to the Soviet public, and in that sense remains a complete nonevent. I shall also assume, and now with the retrospective agreement of all parties, the reality of the United States bombing of Cambodia in 1970, despite official efforts first to conceal the operation and then to deny its rumored existence, less for military security than from fear of public reaction. These events were real, whatever the state of reporting about them.

Now different vocational subcultures have different views of such questions, and it may be a characteristic of the political life to insist on blurring fact and opinion. Perhaps the most vexing triangle of subcultures in the social order is made up of religion, science, and politics. The great misunderstandings along each leg of this abominable triangle are notorious, and no few of these arise from different ways of viewing facts, opinions, and Truth. What distinguishes science is the presumption that there is some core Truth out there which, although never fully knowable, can be approached as a limit, on a staircase of facts. What distinguishes religion is that it is exactly the core Truth which is known, and from within any particular religion, the main problem is the

residual one of deducing the implications of the fixed core of Truth for practices of daily life.

When we come to politics, it must be admitted that the subculture of practicing politicians is not always esteemed for the ways in which it views facts and Truth. Harold Lasswell once put together a collage of quotations about politicians by nonpoliticians over the centuries. Philo of Alexandria, writing in the first century A.D., likened politicians and statesmen to ventriloquists and sorcerers, "men skilled in juggling . . . and tricks of all kinds," and talked of the varied web of political affairs where, "along with the 'smallest possible portion of truth,' falsehoods of every shade of plausibility are interwoven" (Lasswell, 1949). Although Philo may have suffered from an advanced case of political cynicism, his is a view that has echoed down the ages. Facts and Truth are simply viewed more conditionally in the political subculture than in the others, and the conditions are clear: they have to do in the obvious way with whether facts and Truth are agreeable to other ambitions or threaten them.

Nor is it hard to understand the pressures that generate these distinctive views. One is sheer vocational survival. Truth *must* follow function, which is the maintenance of power. At times these postures toward information have an innocuous face. We can admire the agility of the candidate in explaining why an election disaster was actually a triumph, or other forms of blarney and buncombe, especially when accompanied by a twinkling eye. It is said that one inalienable right of the politician is obfuscation, and it may indeed be a necessity.

But there is a darker face to this trifling with information, and it goes well beyond the fact that politicians do not understand why scientists hold raw information sacred, whereas scientists have trouble understanding that the main utility of information is for competitive advantage, be it military or commercial. Add some brute power up front, and we find ourselves moving in the most direct way toward the Orwellian nightmare. What Truth is, is what Headquarters says it is. And if expediency demands, down is up, hot is cold, and white is black.

Before we proceed to a broader canvas, let me add a fourth one to our triangle of subcultures. Undoubtedly the journalistic fraternity is tired of endlessly being counted as the postscript of the Fourth Estate. But it falls to that fraternity to explain the other subcultures to one another. Moreover, journalists from the free media have been at greater pains than anybody else to maintain professional distinctions between fact and opinion. They would prefer first to "tell it like it is," and comment editorially in a separate department. This effort at separation has never been a run-

away success, as I think most journalists would agree. On the other hand, it is hard to argue that there is anything unhealthy about the effort, and once past the first commandment of journalism that the truth not be dull, the views of reality within the Fourth Estate often seem to fit more comfortably with those of scientists than any other.

In politics with a free press, the routine tensions between government and press, once past the peculiar problems posed by wartime, need little detailing. For governments, the media are regularly thought to lack sufficient appreciation of the genius of the administration. They are also, in democracies at least, resented as having power out of all proportion to their nonelective status. How powerful the media actually are is a question that even third parties have not succeeded in defining very well.

We now have collected our main ingredients. With these ingredients in hand, it may be hard to see that there is any mystery whatever in this subject matter. Most Americans would, I suppose, consider the institution of a free press an excellent thing, so there is no particular normative issue to be settled. We might not all be willing to go as far as Thomas Jefferson's celebrated remark, "Were it left to me to decide whether we should have a government without newspapers, or newspapers without a government, I should not hesitate a moment to prefer the latter" (Hulteng & Nelson, 1971, p. 2). Nonetheless, aside once again from the obvious limitations set by security in wartime, it is rather hard to think of appeals to higher values which could possibly underlie the routinized denial of a system of free media. After all, the free press doctrine is a modest one. It does not question the right of governments to broadcast their versions of the news or to justify their ways and good works to their citizens. The government enjoys all of the natural resources of the incumbency advantage, and this is not at all challenged. All that a free press doctrine says is that the power of the state should not be used to prevent other flows of information to its population on matters of public interest.

If there is no normative mystery whatever here, neither is there any motivational one. It is manifestly easier to govern without competing information flows. Still better than having the last word is having the only one. In a world where the only interest is government interest, and it is inconceivable that any citizen or grouping might depart from it even in small ways at the margin, then such prohibitions on media freedom are understandable if, by construction, quite unnecessary. Hence motivation is no mystery.

Finally, there is no capability mystery either. Governments anxious to prevent competing flows

of information typically have the power to do so and make it stick.

And yet, from this cascade of nonmysteries, I found myself plucking one residual mystery. I have already stated it in passing when I noted that it was hard to think of appeals to higher values which could possibly justify the use of force to prevent free media. In wartime, yes. Or if we are satisfied that might makes right, yes. But if there is anything else to be said, I found I could not recollect it.

I regarded even this bit of mystery with some doubt. After all, I am a child of my own political culture, with its great reverence for the Founding Fathers, the mania for checks and balances against absolutist government, and all the rest. I also am saturated with those scientific values that consider direct and unmediated contact with the raw information of reality something quite sacrosanct. Hence for me personal ignorance was as good a hypothesis for this mystery as any, and therefore I set out on a quest aimed at remedying my ignorance. In due time, my quest split into several subquests. The rest of my remarks are a report on these subquests, although most of them can be mercifully brief.

II

My first quest lay toward the classics and history. By classics I mean simply those works that would appear on most lists of the 10 great books in the political philosophy section. These I had largely left untouched since reading them long ago, and it was an interesting exercise to dip back into them with such a sharp focus in mind, and one obviously independent of the reasons why any of these classics were composed or of the historical context that generated them.

I can describe this first quest briefly because it produced little answer to my question. Perhaps a more extensive search could uncover richer lodes than I found. One of the sharpest impressions I received is that in remarkable degree, the whole construct of information seems largely a twentieth-century notion. Plato did develop his celebrated metaphor of cave shadows, addressing the deterioration of information as it is reproduced, but he was dealing with a more general problem. Of course information pervades all of these classic texts by implication, but merely because it is to cognition as air is to breathing. It is scarcely isolated as an entity until studies of propaganda began in our century. I did not, for example, expect much ethical justification from Machiavelli, but I did think he would at least treat deformations of information as an explicit tool of statecraft. In a sense he does, but only as imbedded in

more overloaded concepts such as "flattery" or "fraud."

This relative silence, at least in the major texts I reviewed, may speak to fundamental changes in recent times in the whole macroprocess of information. In an earlier, less interdependent world, persons perhaps witnessed directly many of the events and trends most likely to effect the terms of their lives, and where direct witness did not occur, the means of diffusion, usually oral, were indeed diffuse. It is in the modern world that the events impinging most heavily in a political way upon a citizen's life occur remotely for most people most of the time; furthermore, media channels certainly provide a tempting sort of bottleneck which makes routine government intervention much more feasible than it once was.

The advent of the printing press provided one such handy bottleneck and invited government responses ranging from licensing to confiscation. Such acts helped to crystallize many issues about flows of information and generated a vigorous, if largely unanswered, protest literature. The fact that the Bible was being printed engaged the religious counterpart to the power/information nexus, the ancient question of direct access to the Truth by the rank and file or laity, as opposed to a doctrine that access must be mediated by certified ecclesiastics. Thus, information in sacred texts could in the latter view only be known as revealed by the priests, a proscription that is of course simply the religious version of the issue that is our subject here.

But the concern for direct access, expressed in this period by a whole succession of figures from Martin Luther to George Fox, was hardly new under the sun. You may be aware of the startling view of the early Christian world that scholars have been bringing into focus with the discovery of the trove of Gnostic texts at Nag Hammadi in Upper Egypt. We are all aware that the early centralized Roman Catholic church spent a great deal of time fighting off exotic heresies. What was less obvious was that these heresies were not always isolated challenges, but in part formed a semi-coherent alternative religious community that extended from Lyons in France to Ethiopia, with central norms favoring the direct access of the laity, as well as a participation in ritual so democratic that it even included women on an equal footing (Pagels, 1981). After several generations of hostile coexistence, the hierarchical church at last acquired the police power of the emperor and not only beat down its opponent but also, by outlawing possession of competing gospels, managed to root out most of the information that such a frightening community had ever existed. The Roman bishops were, to draw from Huntington

(1968), "so good at making governments."² The Egyptian collection of illegal texts was buried in the sand in the nick of time.

All things considered, my historical quest had brought meager returns. It was clear that the issue was both ancient and central, but I was still without any considered statements justifying the use of power to cut off access to information by the governed. The closest approach, perhaps, was in church governance, where something approaching an appeal to higher principle or authority could be discerned. However, the principle here pivots on a diety and a vigorous appeal to pure faith, much as does the doctrine of the divine right of kings. These terms may be of great importance in theocracies, but did not much satisfy my more secular quest.

III

Moving to the current period, we might first refresh our memories about the distribution of institutions of media freedom in the world today. The picture here is so simple as to be stark. Media freedom co-occurs at a very strong level with a whole series of other familiar political institutions that make up the syndrome we call "democracy." In fact, the co-occurrence is so overpowering as to make it awkward to deal with media freedom as a separable entity. If a government prohibits freedom of the press, it is likely to deny as well a whole waterfront of civil rights and any organized political opposition or independence of the judiciary. This bundle of civil rights is closely tied empirically to another constellation of institutions having to do with the presence of contested elections. If a government denies freedom of the press, it is very unlikely to have elections, or if some are permitted, either the government reserves the right to disqualify candidates it dislikes, or there is rampant government fraud at the polls.

The civil rights bundle and the elections bundle of institutional traits correlate at a level of about $r = .9$ for approximately 140 countries successfully coded in these terms in the most recent *World Handbook of Political and Social Indicators* (Taylor & Jodice, 1983). Such a correlation undoubtedly errs somewhat to the high side in view of likely contaminations in the original coding (Bollen, 1980). But even with some discounting,

²The Huntington citation has a different referent: "The real challenge which the communists pose to modernizing countries is not that they are good at overthrowing governments (which is easy), but that they are so good at making governments (which is a far more difficult task)" (1968, p. 8).

the polarization of nations of the world into two distinctive patterns is sharp where fundamental relationships between governors and the governed are concerned. We are guilty of very little caricature when we sort the world simply into "open" and "closed" polities. The "open" polities are the conventional democracies. The "closed" polities are the residual assortment of military dictatorships, socialist states of Marxist-Leninist persuasion, theocracies, and other uncharacterizable traditional states.

Such firm monopolies of public information by governments have a variety of other ramifications; I shall take brief note of only two. I assume first that governments of all kinds, both open and closed, are often tempted to take repressive action against inconvenient minorities, actions that at their most brutal extremes are monitored by an organization like Amnesty International. Let me also assume that rectitude and virtue are randomly distributed across governments of the world. Then if it is at all costly before world opinion to be discovered in any human atrocity, it follows that the commission of such atrocities in the fish-bowl of an open political system is a good deal riskier than it would be behind the windowshades of a closed system and hence should occur less frequently.

At the same time, however, closed systems should be able to hide their atrocities more effectively, so it is not clear how a list of *known* atrocities (always a subset of all atrocities) would differ from open to closed systems. If we take a known list, such as may be culled from Amnesty International's recent report on torture (1984), grading the reports for variations in scope and complicity of the central government, and then compare the list with countries arrayed from closed to open by the *World Handbook* measures, the correlation runs in the mid-.30s, meaning more atrocity reports from closed systems. Moreover, such static correlations would surely be outshadowed by dynamic ones for those numerous polities that oscillate between open and closed, because system closing is often a prelude to government-sponsored brutalization and is motivated by it in an obvious way. All told, more information seems to escape from closed systems than their governors would prefer, or perhaps even expect. If the reach for secrecy exceeds the grasp, it is possible that the penetration of a growing world press is in part responsible.

Another nontrivial characteristic of closed polities is their resistance to any domestic research, especially of a social kind, which is not exclusively controlled by the government, and thereby subject to the possibilities for falsification or at least deletion which that power preserves. Within the past 20 years I have been involved with cross-

national research operations of a public kind, collecting data in both the Soviet Union and in the right-wing dictatorship of Brazil during its most repressive phase. In both cases the local part of the research was supervised by established native scholars operating from well-credentialed indigenous research institutions. The panicked responses and threatening pre-emptive measures taken by officials in both countries to the possibility that the raw materials might be accessible to an international research community were indistinguishable.

This is, of course, hardly surprising for anybody who has tried to study closed systems. But among other implications is the obvious difficulty of knowing what truth value to assign to any self-descriptions, such as government statistics, that do see the light of day, particularly on matters dear to national ideology.

The problem of maintaining at least minimal credibility may suggest one fortunate limit to prevent governments from having their unfettered way with flows of public information within their national boundaries. At least since Socrates, there has been sensitivity to the great watershed between what I think of as first- and second-order ignorance, that is, between not knowing something, and not knowing that you do not know it. Intuitively, the behavioral implications of the difference should be substantial: if you are aware of your ignorance in a matter, you will proceed less blithely and will either stop to search for the missing information or, if it is unattainable, will at least behave in a more tentative manner, out of respect for special uncertainty.

This watershed is of importance for closed systems, given a variety of casual evidence at least that there is a certain leaven of cynicism in such systems about official government information, and no little awareness that there is a good deal that the common citizen is not permitted to know. At the same time, we would expect a certain stratification of public opinion in these regards, and skepticism may run out rather rapidly as we move down into these populations. Indeed, working with our data, collected at the height of the Brazilian repression, Geddes and Zaller (1983) found patterns of susceptibility to the anti-democratic rhetoric of the government-controlled media which nicely parallel work we did on information flows many years ago. That is, the relationship between susceptibility and exposure is basically curvilinear, with susceptibility gaining as exposure increases, but only up to a point: it begins to drop off again as we approach the highest-exposure people, who are the best educated and presumably the most sensitive to what they are not being permitted to know. Nonetheless, the population showing sophisticated indif-

ference to government rhetoric was quite limited, and in general, we must suppose that it must take an uncommonly educated populace before governmental control of information flows begins to boomerang in any serious way.

These are some of the empirical patterns. Before I return to my quest for higher normative principles that justify government deflection of information, let us pause to consider what scholars make of these patterns. If we stay global with the open-closed distinction, rather than focus on media freedom alone, then of course a great deal of analysis and interpretation has been carried out. For example, it is well known that closed systems tend to be less economically developed ones, as well as more "traditional." One popular thesis in recent years has been that open institutions are luxuries that polities struggling to modernize can ill afford, both in the sense of up-front administrative costs, and, in the Huntington (1968) formulation, because open institutions encourage an upwelling of public participation that modernizing political institutions may be too weak to handle.

If such theses are correct we could discern one answer to my quest: closing the system may not be a good thing taken alone, but it is better than not modernizing. This answer is worth jotting down, but it is a rather marginal response unless we can also demonstrate that it is impossible to disengage media freedom from other open institutions, since most of the reasons why such institutions are alleged to be a luxury turn out to be a very poor fit for media freedom taken alone.

This poor fit is obvious where up-front administrative costs are concerned. To be sure, to conduct competitive elections fairly costs something, as do some of the other open institutions. But for media freedom the cost issue is exactly reversed. Maintaining the security forces, censorship offices, and other bureaucratic apparatus to ensure unchallenged state mediation of information is remarkably expensive. Letting public information flow as it will is the obvious cost-free default option.

Of course the costs that are more troubling to the regime, although less certain, occur down the road if uncontrolled flows of information about the government become so very unfavorable that the citizenry tries to replace these leaders with others. Once we add a strong *ceteris paribus*, I accept the doctrine that such alterations of leadership can be dysfunctional for progress in development, although the case is clearest where such alterations are, either in the attempt or the completion, bloody and destructive. We are not without social inventions to permit alteration of leadership without bloodshed.

However, if we continue to restrict our focus to

media freedom, it is worth factoring in other costs to development that state control of information is likely to entail. One cost is a loss of government credibility. From our Brazilian data I would suspect that this cost is not visible in more than a small stratum of the population, but that is the group most crucial for development, including parts of a scientific community most offended by state distortions of information. Another kind of cost can arise if the elite insulated from any alteration comes to believe in its own version of the news and, without normal feedback, drifts off into a delusional system that is clearly maladaptive. Surely in recent decades we have seen some spectacular instances of modernization badly derailed because of eccentric convictions about how to proceed with it on the part of elites shielded by their strong political control from the kinds of new information that are unwelcome and hence demand mid-course corrections.

I cannot weigh these costs against any destabilization that minimal respect for independent information might generate. But if the redeeming goal is truly the noble one of development rather than mere maintenance of power, then it is worth pointing out that these other costs do exist, and that they distort badly any simple balance sheet. At the very least, it seems worth matching a concept of political control too weak for the modernization task, with another concept of overcontrol that can become dysfunctional as well.

We should not leave these empirical patterns without recognizing another cultural justification, which says that subject populations in long-term closed systems are not to be pitied because they actually lack any expectation of voice in the system and would be uncomfortable if suddenly given any. A sophisticated version of the same argument is the Leninist doctrine of the vanguard party, which says in effect that party leadership makes the decisions, not the rank and file, because the rank and file do not understand their own real needs, at least in a short run, although a short run that historically, we must add, has never seemed to terminate. From either version it follows that accurate public information is unnecessary, since there is no public decision-making function.

How real people feel about such matters is so obviously an empirical question that it is a pity it cannot be more directly examined. Nor does it inspire confidence that the rulers of closed systems who give these glib assurances are at the same time the most committed to preventing any actual test. This situation produces an astonishing run of public gibberish. This spring, for example, the Iranian government announced that a number of candidates could not be permitted to run "because they had fallen out of touch with the

people." Even counterelites of some popular liberation fronts, whose main claim to legitimacy is representation of the people against tyranny, threaten to shoot up the countryside to prevent actual people from being asked their preferences at the polls, and escalate threats of disruption even farther when it is suggested that some outside party, like the United Nations, be brought in to assure the fairness of the election.

My own speculation is that these accounts of real people gladly abdicating their own preferences because cultural or ideological arguments tell them they must are grossly false. The scattered instances of relevant data I have seen would certainly bely such descriptions. At least some common subjects in closed systems are feistier than elites often want to make them out to be, and while my documentation is sketchy, I read in the fright of elites and counterelites at any actual test that they share my intuitions.

Is it not this feistiness which translates in the most direct way into the kind of penchant for disorder that Huntington and others find detrimental to development? In one sense yes, but in another sense, not necessarily. There is some truth here because subjects do become aggrieved, and if given any opening, are glad to support counter-elite cadres who will do battle for them. On the other hand, there is substantial evidence that in many of these episodes, the common people tire of vicious public disorder much more rapidly than the elites locked in the struggle over power. There is surely nothing to astonish us that this might be so: the common people often suffer much more trauma in such disorder than the elite combatants do. So the elites carry on the power struggle, now quite autonomously, agreeing on little save that the actual people are not to be consulted in the matter. In the degree that this version of the scenario does take place in some cases, it is a pity to say generally that open institutions invite a paralyzing disorder. Their absence can also help prolong the agony.

Another current arena that seemed worth examining in search of ethical justifications for government suppression of public affairs information is the heated debate which has persisted over the past eight years or more, especially within UNESCO, concerning what has been championed by a large number of Third World countries as a "New World Information and Communication Order" (NWICO). One impetus behind this movement is a discouragement within many developing countries at the fact that all of the large international press services are headquartered in the developed countries and thereby report news from the developing countries in what is seem to be an unflattering and often alien perspective. What is sought is better balance in the reporting of world

news. The problem is how to move toward such balance within the framework of a United Nations committed from the outset to freer flows of information in the world, including freedom of the information media.

In abstract principle, when an imbalance develops because A has outrun B, it may be redressed either by expanding B or by constricting A, or some combination of the two. The expansionary redress in this instance, which involves helping with the enlarging of communication infrastructures in less developed countries through both technical aid and the reduction of economic and financial obstacles, is not subject to controversy and has been an explicit element in United Nations programs more or less from the beginning (see Green, 1956). Such expansions are part of the NWICO vision. Some countries however seem less interested in such expansionary solutions than in resolutions that constrict the flow of news by establishing various forms of local political control over it.

Debates over the NWICO vision are rather more confusing than might appear because although a set of general goals has been proposed, actual steps of implementation have frequently been left to be worked out, and therefore their potential meaning remains unclear (Sussman, 1984). Moreover, these debates generate less in the way of ethical justifications for governmental interceptions of information than might appear because the goals usually stated for the establishment of controls are ones of promoting freer flows of information rather than constricting them. Thus one of the clearest tangible proposals to date in the specification of NWICO is for the licensing of journalists by governments. It is billed as a step that would liberate information because the journalist, once licensed, could work "secure" from government harassment. The fact that interest in what seem actually to be restrictive proposals comes predominantly from member states whose governments flout United Nations ideals of media freedom in their own domestic practice does not reassure the observer about the actual intent of steps proposed. However this may be, we search again in vain here for ethical justification of governmental media control. Most countries express support for UN ideals of media freedom, but there is marked variability in views of how such noble goals are best implemented.

IV

The last stage of my quest is again brief, and in a sense, light. I had genuinely imagined at the outset that I could be guided to some rationale beyond naked power for state prevention of independent media. Lest this seem naive, let us

remember that Mosca and other sage observers have argued convincingly that elites have a great capacity for justifying institutions they find useful. Granted that this takes time and routinization: governments are often thrown by circumstance into quick aberrations that are left unexplained. Granted also that the Mosca view is cynical enough as it stands: the dirty grit of necessity comes first, and the pearl of justification only forms in due time to reduce friction. Nonetheless, sooner or later some justification is to arise, and given the importance that information access would seem to have for human dignity, I wanted to see what such justification might be.

Or, to put the matter more simply, imagine a friendly backyard conversation in which a government member sets out to explain to his next-door neighbor why it is in everybody's interest to use part of the citizen's taxes to prevent him from receiving some kind of information about his world. In such a conversation, by construction friendly and relaxed, just what would be said?

I had not searched everywhere for justifications. After all, military dictatorships, while keen on monopolizing public information, tend to come and go in rather short stints and in any event rarely feel much obligation to look beyond naked power as a reason for governance. Human dignity is just not the highest priority. I did not look much to theocracies either, because as we have seen, that answer was not too satisfying for the modern world.

What this left as the most definable residue of closed states were Marxist-Leninist governments. These seemed ideal. They are not as transient as military interludes, so that justifications have had time to form. Moreover, these states place great emphasis on human dignity, and insist on a basic role for normative theory in matters of politics and society. Finally, state monopoly on information flows is a very central part of the blueprint for governance in these states, not just in wartime or under duress, but as a routine matter. Indeed, if one takes what are usually called the stable governments of the world, strict state control of public information is a more sharply distinctive characteristic setting apart Marxist-Leninist governments than anything else commonly coded, such as economic distributions. In practice, if not necessarily theory, this information control is simply the defining signature of such states.

Therefore I sought references to the relevant theoretical literature from colleagues familiar with and, more often than not, personally sympathetic to, current Marxist thought. This turned out to be a remarkably blank wall. One sympathizer could hardly understand my question. After second and third rephrasings, he asked in return with some impatience how there might conceivably be any

justification for such practices. He went on to say that undoubtedly some East European apologist must have written something on the subject, but it certainly was no part of the central Marxist literature. I also received directions to sympathetic descriptive material on the structure and practices of the Soviet press, but with forewarning that it would not address my questions. It did not.

Other efforts at more substantive answers to my query tended to be one or another form of non sequitur. For example, I was told I should not worry about Communist citizens being shut off from world news because much of that simply defied control, for reasons ranging from sailors in foreign ports to European airwaves. I have already assumed this to be true: individual citizens, in an implacably Protestant way, do have their own unmediated interactions with the environment. They do find news and pass it on, and governments, however hard they may try, cannot always stand fully in the way of it. Nonetheless, the limitations on success scarcely address my question about what principles are induced to justify the effort.

I have saved one other response for last because, while again a non sequitur in its own way, it provided as much food for thought as all of the rest of the responses put together. I encountered a television interview between a western journalist and a high official of the Soviet news hierarchy. The journalist more or less asked my question, wondering why the Soviet government permitted no domestic press freedom. The official responded that this was not a very constructive question, because capitalist societies had no free press, either. The journalist began to protest, but the official went on to say that it was self-evident there was no free press, because what was claimed to be a free press was not in fact dedicated to the overthrow of capitalism.

This provides us with a fascinating definition and perception which may explain a good deal, albeit of little help to my quest. It seems to be presumed that a free press is some monolith in ideological terms, instead of the enormous scatteration of positions and nonpositions that arises in the West, with a spectrum ranging from organs indeed dedicated to the overthrow of capitalism at one pole all the way to organs that are its staunch defenders, with the great middle range occupied by organs that accept the basic structure of capitalism but, as businessmen bitterly lament, carp persistently at its excesses. Across all of this spectrum, the survival of specific voices depends, if not exclusively at least in significant degree upon maintaining a clientele that resonates to its posture, or upon not offending that large clientele that hopes to order mere news, going light on the ideological garnish.

The official has a totally different vision of what institutions of media freedom actually mean. If we see things from his view, it is clear that accepting media freedom would open his system to information organizations single-mindedly dedicated to ripping Communism out root and branch. If that is the conception of the challenge, then it is not hard to see why it would be unwelcome on the face of it. But my own prediction of what would happen in such an improbable event are quite different. I would wager very heavily that such a true opening would produce not just one voice, but at least several and probably very many. I would also wager, although more cau-

tiously, that the great middle of such an emergent spectrum of free voices in his country would accept communism as a basic structure, but would carp persistently on its excesses and would in general make governance more difficult.

Perhaps this is a prospect sufficiently sinister that it could not be considered. On the other hand, I for one would appreciate hearing the normative side of the rejection, rather than the pure defense of acquired power. To me, the choice implied in a varied stream of information about one's environment is a minimum requirement for human dignity, and one that power should not be used to eliminate.

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A New View of Political Accountability for Economic Performance

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Most political support models imply that in evaluating economic performance, voters use a standard that would provide poor predictions of the future and leave the economy vulnerable to manipulation by vote-hungry politicians. Drawing on macroeconomic theory, we develop a simple standard of evaluation which encompasses a concern not only for current economic outcomes, but also for accurately assessed future consequences of current policies. We find that political support for the president can be explained as well by models that assume that voters use this sophisticated standard as by models that assume voter naivete.

Our analysis questions the wisdom of measures typically used to assess voter evaluation of economic performance in a variety of theoretical contexts. The results also help to explain the absence of convincing evidence that governments exploit voter ignorance in manipulating the economy.

Elections in representative government are simultaneously an opportunity for voters to hold current incumbents accountable for their past performance and to choose among contenders for future office. A growing body of scholarship has emphasized the retrospective evaluation of incumbents as a basis for electoral choice, in part because it seems to be commonly used by voters, and in part because it is seen as a sensible guide to decision.¹ One reason that retrospective evaluation is seen as sensible is that insofar as the past performance of incumbents is a good indicator of their probable future performance, retrospective voting combines the two tasks of holding incumbents accountable and predicting the future.

We will show that the basis for using the past to project the future used in most of the literature on retrospective evaluation of economic performance can be misguided. We present an alternative, more sophisticated standard and show that it explains popular support for the president about as well as, and in some ways better than, the usual standard.

The first section of the article reviews the rationales for retrospective voting, and the second section draws on macroeconomic theory to develop a new standard for evaluation. The third section presents the political support function we use for empirical comparison of sophisticated and naive voter standards, and the last section presents and discusses the results.

Existing Rationales for Retrospective Voting

Downs (1957) and Key (1966) have expressed leading rationales for retrospective voting, the central distinction between them being whether or not the past is used as an indicator of the future. Key makes the more cautious claim. He uses data showing that voters stay with a party or switch on the basis of assessments of past performance to make "the perverse and unorthodox argument . . . that voters are not fools." He makes few positive claims about electoral control and aims his argument at the then-fashionable view that the American electorate is "straightjacketed by social determinants or moved by subconscious urges triggered by devilishly skilled propagandists"

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¹See especially Downs (1957), Fiorina (1981), and Key (1966).

(1966, p. 7). He makes no claim about control of the future; he contends only that voters respond pragmatically with approval or disapproval of what has taken place.

Downs is more ambitious, claiming that "rational behavior is impossible without at least some way of forecasting future events" (1957, p. 106). He defines the logic of voting in terms of a comparison of future utility expected under alternative parties. "Since one of the competing parties is already in power, its performance in period t gives (the voter) the best possible idea of what it will do in the future" (p. 39). Downs acknowledges that this claim assumes that the incumbent's policies will have some continuity, but he argues that the self-interest of the parties will assure "reliability, integrity and responsibility" (pp. 103-109).

Neither Key nor Downs addresses the question of what sorts of standards are or should be used in their rationales for retrospective voting. We will show that the standard used is important, whether the past is used to predict the future or not. Before making this argument, we review briefly a few studies of the relationship between economic performance and political support which are especially relevant to our discussion of standards.

Kramer (1971) developed a careful rationale for retrospective voting which was inspired by Downs and which may be seen as an application of Downs. Kramer rejected as unrealistic a view in which voters choose after comparing party platforms and other information and proposed instead that a more relevant decision rule would be to use the past performance of the incumbent party as a readily available indicator of what it would do if reelected. Voters are assumed to support the incumbent party if its record is deemed satisfactory according to some simple standard the electorate expects it to meet and to oppose it otherwise.

Kramer observes that because it is not possible to observe voters' expectations directly, "a reasonable and convenient hypothesis is that expectations about year t are formed on the basis of experience during the preceding year" (1971, p. 134). As operationalized, Kramer's performance variables are percentage change in real and nominal income, the rate of inflation, and change in the level of unemployment, all measured in the year before the election.

A great many subsequent studies have followed Kramer's lead in relating political support to economic performance, offering many useful refinements. However, very few have been as self-conscious and careful about inferring expectations of the future. Three studies of political support make use of measures of expected future economic performance, but none identifies the processes by which these expectations are formed,

and none derives them from past performance.²

Rabinowitz and Zechman's (n.d., pp. 5-7, 14-15) study of presidential voting addresses directly the question of whether voters consider "the implications of current policies for future performance," and they include in their presidential vote model a variable for expected future performance. They argue that "a growing body of research has indicated that individuals apparently do have reasonably accurate expectations about short-term fluctuations in the performance of the economy and they act upon these expectations." Accordingly, they use actual change in real per capita income during the year following the elections as an unbiased proxy for expectations of future performance; that is, they use post hoc observations of the "future" to measure expectations of the future.

Monroe and Levi (1983) use directly observed economic expectations to explain variations in popular support of presidents, but their inference is only slightly less indirect than that of Rabinowitz and Zechman. Specifically, they use the Joseph Livingston time series of data from surveys of leading businessmen and economists on their expectations of future inflation and economic growth. Although the assumptions are not entirely clear, these elite attitudes seem to be taken as proxies for popular expectations.

Kuklinski and West (1981) report direct survey evidence on voters' own assessments of both their past and their expected future financial well-being. They are thus able to assess directly the relationship between the two in the 1978 National Election Study, from which their data are derived. That relationship was positive, but weak. Kuklinski and West's finding is a sobering one, because it implies that perceptions of past performance may be poor indicators of expectations of the future. It may also undermine Kramer's use of objective measures of past performance to infer expectations of the future, without giving a hint about how one might use the past to infer future expectations more accurately.

These studies are important in providing evidence that expected future well-being is significantly related to political behavior, and they sup-

²In addition to these more narrowly focused studies, Alt (1979) makes great use of expectations in general in explaining the electoral "politics of economic decline" in Britain since 1964, and he makes a careful effort to explain observed expectations of inflation, in particular, with past inflationary experience (1979, chap. 4). Alt's work (1979, 1981) is a rich source of thinking on expectations and a compelling case for their importance in political analysis. See also Minford and Peel (1982), Chappell (1983), Chappell and Keech (in press) and Paldam (1981).

port the notion that voting and other manifestations of political support are at least partly prospectively oriented, as Downs would have predicted. However, these studies leave important issues unaddressed. They do not explain how expectations are formed or related to past experience, and they do not reflect the insights of macroeconomic theory for constraints on possible outcomes, for the possibility that incumbents may not be fully responsible for current outcomes, or for the possibility that good times in the long run may require some short-run distress. We propose and test a model that incorporates these features, at least approximately.

The Insights of Macroeconomics for Measuring Economic Performance

The literature on mass political behavior noted above does not give us clear indications of how, if at all, voters use information on the past to form expectations of the future, but the literature on macroeconomic theory suggests how they might. We will use macroeconomics to show why simple extrapolations of the future from past values of economic variables can be misleading and to identify a simple standard by which voters can combine an assessment of past performance with predictions of future performance.

To know how well an administration performs in managing the economy, we need to understand what patterns of outcomes are feasible and to define a standard of desirability over feasible outcomes. Macroeconomic theory will help us to identify sets of feasible outcomes, and we will present a standard that will reflect a range of reasonable voter preferences given some knowledge of the nature of constraints.

Although there is considerable debate over what macroeconomic theory best describes reality, we believe that the two important assumptions that we make about the economy reflect mainstream views in economics. First, we assume that policymakers can systematically affect economic outcomes in the short run. This point is particularly controversial in current macroeconomic thought, but some theoretical arguments and most empirical evidence seem to support it. This is of course an implicit assumption in all previous studies of political support functions as well. Second, we assume that the tradeoff between unemployment (or output) and inflation differs in the long and short runs.

Although output may be increased (or unemployment decreased) at the cost of higher inflation in the short run, many economists deny that such a tradeoff is possible in the long run. They argue that the long run Phillips Curve is vertical; that there exists a natural rate of unemployment (or

output) that can be associated indefinitely with any rate of inflation. Thus, expansionary monetary and fiscal policies may be able to reduce unemployment below its natural rate at the cost of higher inflation in the short run, but the new combination of lower unemployment and higher inflation is not sustainable in the long run. Once economic agents anticipate the higher rate of inflation and those anticipations are reflected in contracts and market prices, the output-increasing effects of the stimulus are neutralized. Unemployment and output return to their natural rates, while the inflation rate remains higher. Similarly, contractionary policies temporarily increase unemployment and lower output, but in the long run these variables return to their natural rates at a lower inflation rate. The central point is that tradeoffs that are feasible in the short run evaporate in the long run.³

The practical implications of this result for sensible voting and for the literature relating political support to past economic performance are profound. Almost all of that literature follows Kramer in assuming that voters simply punish incumbents for inflation and unemployment and reward them for growth of output. This behavior would be sensible if inflation, output, and unemployment could each be controlled independently of the others, and if current short-run values of these variables were accurate proxies for their future long-run values. Our preceding arguments have indicated that this is not the case, however. We term such behavior *naive voting* because it implies that voters are unaware of constraints relating outcomes at a given point of time or across time and provides vote-maximizing

³We have consulted five recent editions of intermediate texts in macroeconomic theory to see how these questions are typically handled. The five texts were Edgmand (1983), Dornbusch and Fisher (1984), Maisel (1982), Gordon (1981), and Froyen (1983). All five texts present the natural rate hypothesis (implying a vertical long-run Phillips curve) as an accepted view. Regarding the implications of rational expectations theories, four of the five texts argued that "policy ineffectiveness" was of dubious validity, based on either empirical findings or theoretical arguments about the inappropriateness of market clearing assumptions. The fifth, Froyen, presented a balanced but agnostic view on that opinion. Most criticism of the results of the rational expectations models were not directly critical of the notion of rational expectations—rather they criticized the associated assumption of continuous market clearing. This leaves open the possibility of assuming rational expectations on the part of voters and economic agents, without assuming away the possibility that policies can systematically affect outcomes. See for example Fischer (1977). This is a view compatible with the sophisticated voter model presented in this article.

politicians with opportunities for manipulation of the economy. Nordhaus (1975) recognized this when he showed that a "political business cycle" could be induced by a government that maximized a voting function very much like those appearing in the literature. Specifically, if voters predict future unemployment and inflation rates by extrapolating from their past values, politicians have an incentive to create apparently desirable, but unsustainable, combinations of inflation and unemployment just before the election. Theory indicates that cyclical manipulation will be most pronounced if voters have short memories, but it may also occur even if voters value or remember all portions of the term equally well.⁴

In contrast to the naive voters characterized by most of the literature, a sophisticated voter would have some sense of feasibility constraints in evaluating outcomes and would be concerned with future as well as current implications of present choices. He would reward incumbents for selecting desirable policies even when times are bad and punish them only for those undesirable outcomes for which they could reasonably be held responsible. For example, an incoming administration might inherit high rates of inflation from its predecessor. Because current inflation is to a large extent a result of past expectations of inflation, the new president should not immediately be punished because of the continuing inflation. Indeed, desirable policy in this instance might call for contractionary policies aimed at gradual reduction of inflation in the long run with modest temporary sacrifices of output. Sophisticated voters would neither punish the president for the inflation (which was caused by predecessors), nor would they reward expansionary increases in output which could lead to further accelerations of inflation in the future. In short, sophisticated voters would recognize that short-run choices are constrained by economic possibilities, and they would reward or punish according to whether selected policies would promote movement toward desired long-run outcomes.

Although the requirements for sophistication may seem very demanding, we can construct a simple measure of economic performance which incorporates the necessary criteria and at the same time might be approximated by ordinary citizens. We will assume that the president, through fiscal and monetary policies, controls real output, Q .⁵ We then define the variable Z_t to serve as an indicator of policy stance, where

$$Z_t = Q_t/QN_t - 1,$$

and QN is natural real gross national product, a level at which there is no tendency for inflation to accelerate or decelerate (Gordon, 1981, p. 11 and Appendixes B and C). When Z_t is greater than 0, output exceeds its natural rate, indicating expansionary macroeconomic policies. When Z_t is less than 0, output is below its natural rate, indicating contractionary policies. A simple linear feedback rule relating Z_t to the lagged rate of inflation, $\dot{P}_{t-1}$, is used to define desirable policies:

$$Z_t^* = d\dot{P}_{t-1}, d < 0.$$

Z_t^* denotes the optimal value of Z_t as perceived by sophisticated voters. This rule simply states that the higher the prevailing rate of inflation in period $t-1$, the more contractionary policy should be in period t . The slope parameter, d , will be larger in absolute value the greater voters' aversion to inflation relative to recession. In the long run, adherence to such a rule would lead to zero inflation and growth of output to match its natural rate (ignoring the effects of stochastic shocks).

To measure economic performance, then, it is appropriate to judge policymakers on the basis of how far they deviate from the optimum policy Z_t^* . In our empirical work, a performance measure appropriate for sophisticated voters is provided by:

$$S_t = |Z_t - Z_t^*| = |Z_t - d\dot{P}_{t-1}|.$$

Higher values of S imply worse economic performance. This standard of sophisticated evaluation of economic policy may seem complicated, but it is merely a mathematical approximation of a simple rule: voters punish inflationary and unsustainable expansions, and they accept recessions only as a means of reducing inflation.

Ordinary citizens might well be able to use such a rule without being implausibly sophisticated, because the rule is not far removed from common sense. Consider first the fact that it punishes incumbents when the economy is pushed beyond its capacity, or overheated. We would not expect citizens to demand economic expansion when most of the economy is working at capacity. Furthermore, the idea that too much economic activity may be inflationary is a common aspect of public discourse and presumably not beyond the ordinary citizen. Certainly the demand for expansion would be less when the economy is at capacity than when it is in recession.

⁴See Chappell and Keech (1983).

⁵This simplifies reality, since presidents actually have only partial and indirect control of Q . However, it is

weaker than assuming direct control over unemployment, inflation, and output as other voting models implicitly do. In addition, we later consider modifications of the model which relax this assumption.

The other main feature of our sophisticated decision rule is that it implies that voters will be more tolerant of recession when inflation is high than when it is low. This too is only a minor extension of the widely discussed idea that recessions are likely to bring down inflation. In general, the sophisticated decision rule is an operationalization of the simple idea that citizens do not respond to inflation or income growth indiscriminately, regardless of the value of the other.

For purposes of comparison in the empirical analysis, we will also make use of a simple measure of performance appropriate for naive voters:

$$N_t = \gamma_1 \dot{P}_t + \gamma_2 Q_t / QN_t.$$

Here performance is simply measured as a linear combination of the inflation rate and the output ratio Q_t / QN_t . The conventional hypotheses would predict that voters would punish higher inflation ($\gamma_1 < 0$) and reward higher output ($\gamma_2 > 0$).⁶

The Political Support Function

Our main concern is to compare naive and sophisticated standards by which voters may evaluate economic performance in order to make inferences about political accountability and popular control of elected officials. Presidential approval polls are especially useful for evaluating these questions. They offer rich time series covering long periods with varied economic conditions. These polls are also useful in that they focus directly on performance. The polls simply ask respondents "Do you approve of the way that _____ is handling his job as president?" whereas voting requires comparisons with alternative candidates who may vary in attractiveness and thus not provide a constant benchmark.⁷

Even though our main interest is in the standards voters use for evaluation of economic performance, we need a fully specified model of political support in order to address this question clearly. In developing such a model, we have drawn on a rich body of literature for guidance about how to handle a variety of issues. These issues may be grouped into two major categories: 1) accounting for the ways that time can influence

the evaluation of performance, and 2) determining how noneconomic determinants of support should enter the model. In this section we introduce the general political support model to be used in the empirical analysis and discuss its implications for these issues. The general form for the sophisticated model is

$$POP_t = f(T_t^*) (\alpha_0 + \alpha_1 \bar{S}_t) + X_t \beta + e_t, \quad (1)$$

whereas that for the naive model is

$$POP_t = f(T_t^*) (\alpha_0 + \alpha_1 \bar{N}_t) + X_t \beta + e_t. \quad (2)$$

$\bar{S}_t$ and $\bar{N}_t$ are weighted averages of the economic performance standards, X_t is a vector of noneconomic variables, and T^* is the number of quarters the administration has been in office.⁸

Time

The Weighting of Different Periods of Economic Performance. In our models the weights attached to lagged values of the economic performance measures reflect two separate phenomena: first, the increasing importance attached to average performance as time in office accumulates, and second, the selection of relative weights for past versus present performance in computing that average. The first of these is represented by the term $f(T^*)$. When a president first takes office, citizens have little evidence to aid them in evaluating economic performance. The president will not have had time to put his stamp on economic policy, and there will not be much experience to evaluate. But as time in office accumulates, respondents obtain more information and can be expected to attach greater weight to it, so $f(T^*)$ should be an increasing function.

Accordingly, for empirical purposes we specify that

$$f(T_t^*) = \sqrt{T_t^*}.$$

Over eight years, $\sqrt{T_t^*}$ will increase at a decreasing rate from one to four at the end of the first term and almost six at the end of the second. Although this is admittedly a somewhat arbitrary specification, we believe it reflects a sensible way for voters to take into account the unreliability of early information, whatever their standard of evaluation.

The relative weights attached to past and present performance are incorporated into $\bar{S}_t$ and $\bar{N}_t$.

⁶The output ratio is closely related to variables used in many other support models: output or income growth or the unemployment rate. See Gordon (1981, pp. 297-299).

⁷Of course voters may implicitly make comparisons when responding to the polls, especially when elections are imminent. This may account in part for cyclical patterns in the popularity series which are discussed later.

⁸We will consider the Kennedy-Johnson and Nixon-Ford presidencies as single administrations for this purpose.

Both performance measures are modeled as weighted averages and computed over the history of the administration:

$$\bar{S}_t = \frac{\sum_{k=0}^{31} |Z_{t-k} - d\dot{P}_{t-k-1}| D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \quad (3)$$

$$\bar{N}_t = \frac{\sum_{k=0}^{31} (\gamma_1 \dot{P}_{t-k} + \gamma_2 Q_{t-k}/QN_{t-k}) D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \quad (4)$$

where $D_{t,t-k} = 1$ if the administration in office in quarter t was in office in quarter $t-k$, $D_{t,t-k} = 0$ otherwise, and $0 \leq g \leq 1$.

The dummy variables $D_{t,t-k}$ assure for both models that the performance of previous administrations does not count in the evaluation of incumbent administrations. The parameter g attaches weights to past outcomes, allowing voters to forget or discount the past. If $g = 1$, each performance measure is a simple average over all quarters of the current administration. If $g = 0$, only the current quarter is evaluated. All values of g between zero and one imply weights that decline geometrically as performance recedes further into the past.

The extent to which voters remember or weight past periods is of limited consequence in the sophisticated voter model, because in each period the performance measure takes into account what kinds of policies are sustainably feasible and therefore the relationship between present policy and the future. The sophisticated voter is not vulnerable to manipulation by vote-seeking incumbents, regardless of the length of his memory.

However, for the naive voter model, the length and weighting of the evaluation period have more important implications. A long memory would reduce the incentives for incumbents to induce early term recessions or preelection expansions whose inflationary costs are postponed until after the election. But a long memory is only a partial substitute for a sense of feasibility constraints. To be invulnerable to this kind of manipulation, the voter needs the kind of ability to anticipate the future consequences of present policies which is provided for by our sophisticated voter standard.

Our political support function is novel in distinguishing voter memories from the weight assigned to the incumbent's performance over time. In other models (Chappell, 1983; Hibbs, 1982a, b, c), geometrically declining weights were specified for periods further in the past, in order to reflect voter discounting or forgetting of past

outcomes. But by not separately specifying the possibility that average performance might be weighted more heavily as time passes (regardless of the pattern of weights used in computing the average), previous models have confounded the two patterns.

Honeymoons and Other Time Trends. Our $f(T^*)$ function imposes an assumption that voters attach a lesser weight to average performance early in an administration's term. We also expect that voters may offer new incumbents a honeymoon period early in a new term, such that the new president will be popular for a time in the absence of a performance record that can be taken seriously. Our model permits the possibility that voters will give a new president a benefit of the doubt until he has a record that can be evaluated.

We model the early-term honeymoon by including in the vector X_t six dummy variables indicating each of the first six quarters in office for a new administration:

$P_k = 1$ if the current administration has been in office for exactly k quarters, otherwise $= 0$; $k = 1, 2, \dots, 6$.

Inclusion of the series of dummies allows flexibility in the specified pattern of honeymoon effects, subject only to the restriction that honeymoon effects are exhausted after six quarters in office. This restriction seems reasonable, because midterm elections are imminent at this point. We expect the coefficients associated with these dummies to be large in the first quarter and to decline as time in office accumulates.⁹

The final time-related feature involves the parameter α_0 , which permits the model to pick up secular trends in popularity, and, following Kennell (1978, p. 520) serves as a "diagnostic for detecting whether any trend producing variables have been left out." This choice is to be contrasted with those of Mueller (1970, 1973) and Stimson (1976), who used functions of time as direct measures of variables meant to allow tests of abstract theories explaining change in presidential popularity. It is to be contrasted also with Hibbs (1982a, b, c), who does not include any trend or independent honeymoon variable, but models performance in such a way that any trends would be picked up in the performance terms.¹⁰

⁹We count the first quarters of Johnson's and Ford's presidencies as new terms for this purpose.

¹⁰However, Hibbs and Vasilatos (1981) include trend variables. In an exchange following (Hibbs, 1982b) he defends his view that any honeymoon must be performance related.

Noneconomic determinants of popularity

We propose that two other dimensions are of sufficient importance that they deserve inclusion in the model: war, peace, and international security, and the corruption or rectitude of the administration. Unfortunately, the relevant data from which indexes of performance on these determinants of popularity might be derived are not easily identified, and neither dimension is as continuously monitored as economic performance is.

For the international security dimension, the presence and intensity of a war effort are surely among the most salient indicators of performance, but how they should be evaluated is less obvious. For a pacifist, war is an unambiguous bad, but for others there may be circumstances under which a war effort is a basis for approval rather than disapproval. Of course, some wars are more popular than others, and the amount of support for a war is likely to be dependent on its cost (Mueller, 1973, chaps. 7, 3).

There was only one war, Vietnam, in the period of our study, and it is widely acknowledged to have had a negative impact on support for the presidents under which it was fought. We include a measure for war and its costs in the form of a quarterly measure of the number killed in action in Vietnam.

For the corruption dimension, the Watergate affair is obviously the most important phenomenon. For lack of a better way to quantify the varying intensity of the corruption issue, we have simply used a dummy variable equal to one for 1973 IV through 1974 III, the period from the Saturday night massacre until Nixon's resignation.

Both of these choices are consistent with the handling of noneconomic issues by other analysts. In contrast to Hibbs (1982a), however, we have chosen to use only current values of the noneconomic variables. Vietnam and Watergate are qualitatively different from economic performance, and these differences may make it sensible to evaluate past values of each in different ways. The problem of managing the economy recurs in much the same form in every period. In contrast, political problems are more often unique and nonrecurring, so that past performance may not have much predictive power regarding how future problems will be handled. Although we specify only a current effect of political variables in the models developed in the text, an alternative assumption is employed in sensitivity tests to be described in the Appendix.

Several studies have included measures of more idiosyncratic events, usually designated as rally points. We choose not to do so because we expect these events to have varying effects. In the

absence of further a priori information about anticipated effects, we believe that rally events should be viewed as random perturbations which do affect popularity, but in unpredictable ways. Such variables are appropriately left in the equation's error term.

Finally, we include an intercept dummy for each president which is meant to estimate his initial popularity level. These, along with the Vietnam war, Watergate, and honeymoon variables, are included in the vector, X_t , of noneconomic variables.

Empirical Analysis

In this section we describe the results of estimating political support functions of the general form described in the preceding section. Measures of economic performance appropriate for naive and sophisticated voters will be used alternatively, and their relative abilities to explain presidential popularity will be noted. In addition, we summarize the results of sensitivity tests undertaken to see if the major implications of our tests are robust with respect to modifications of the political support function.

The dependent variable, POP_t , is the fraction of Gallup Poll respondents who answered favorably to the approval question quoted above. POP_t is computed as an average of polls taken during the appropriate quarter. In addition to estimating equations to explain aggregate poll data, we have also estimated equations for specific subgroups based on occupational, educational, and partisan classifications. The analysis of these disaggregated groups is intended to provide additional information about the general applicability of the models, as well as to identify different preferences across groups. The time series analyzed for the aggregate poll data covers the 1957 I to 1980 IV period, whereas the group data cover somewhat shorter subperiods.¹¹ Q and QN are measured respectively by real GNP and Gordon's estimates of natural GNP. The inflation series is an annualized rate computed from the GNP deflator.¹² The remaining variables were described in the preceding section.

The equations to be estimated are found by

¹¹The data for partisan and occupational groupings cover the 1961I to 1979IV period; the data for educational groups cover the 1965I to 1980IV period. Each of these series lacks observations for several quarters when polls were not taken.

¹²The data series for QN is from a mimeographed supplement to Gordon (1981). The supplement reflects revisions made in the data series after publication of the book. Other economic data are from the CITIBASE Economic Database.

substituting equation (3) into equation (1) and equation (4) into equation (2). The sophisticated voter model is

$$POP_t = \sqrt{T_t^*} \left[\alpha_0 + \frac{\alpha_1 \sum_{k=0}^{31} |Z_{t-k} - d\dot{P}_{t-k-1}| D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \right] + X_t \beta + e_t, \quad (5)$$

and the naive voter model is

$$POP_t = \sqrt{T_t^*} \left[\alpha_0 + \frac{\sum_{k=0}^{31} (\gamma_1 \dot{P}_{t-k} + \gamma_2 Q_{t-k}/QN_{t-k}) D_{t,t-k} g^{t-k}}{\sum_{k=0}^{31} D_{t,t-k} g^{t-k}} \right] + X_t \beta + e_t, \quad (6)$$

where $\gamma_1' = \alpha_1 \gamma_1$ and $\gamma_2' = \alpha_1 \gamma_2$. These equations are nonlinear in the parameters, but can be estimated by nonlinear least squares. In estimating the sophisticated voter models, convergence problems forced us to impose values for the weighting parameter g and then estimate the remaining parameters using a nonlinear least squares algorithm. A finely subdivided grid of possible values for g was examined, and results are reported for the value of g which minimized the sum of squared residuals. This procedure provides nonlinear least squares estimates, but does not yield an asymptotic t -statistic for the estimate of g .¹³ Because our results indicated that serial correlation was generally present, in all cases we have reestimated the equations correcting for the presence of first-order serial correlation.¹⁴ For the aggregate data we present both corrected and uncorrected equations, but to conserve space, only

¹³In a few regressions it was necessary to search a grid over both g and d . The absence of a t -statistic for d in the tables indicates that this was the case.

¹⁴A two-step procedure was used. First, the residuals of the uncorrected nonlinear least squares estimates were used to obtain an estimate of the error term correlation coefficient. then we reestimated assuming the presence of first order serial correlation with the error term correlation coefficient set equal to the calculated estimate.

the corrected equation estimates are provided for the group data regressions.¹⁵

Table 1 provides complete regression results for sophisticated and naive voter models using aggregated popularity data; estimates of the key economic parameters and summary statistics are provided in Table 2 for the group regressions.¹⁶ The results show strong support for the sophisticated voter models. In all equations for aggregate and group data the parameter α_1 is negative, indicating that voters punish suboptimality as defined by our sophisticated standard. In addition, the estimates are significantly different from zero at the .05 level or better in most cases. Estimates of d are always negative and large in absolute value, indicating strong aversion to inflation. Indeed in some cases the estimates of d are probably unreasonably large. For example, in the corrected equation for the aggregate data, the value of $d = -.019$ would imply that if last period's inflation rate were 10%, voters would like to see this period's real output be 19% below its natural rate in order to combat the inflation. The estimates of d also tend to be large in the group equations.

Although the excessive magnitude of the d estimates tempers support for the sophisticated voter model, it certainly does not refute it. Although the estimates are large, so are standard errors; d is significantly different from zero at the .10 level in only about half of the equations and is never significantly different from what we would consider plausible values for d (for example $d = -.010$). Hence the results are quite consistent with a priori expectations.¹⁷

¹⁵Several econometric caveats should be noted. First, the popularity series are missing several observations (for quarters in which no polls were taken), which implies that the correction for serial correlation is only approximately correct. Since there were generally only a few missing observations, this is of minor consequence. Second, the reported standard errors are based on an implicit assumption that values for ρ , the error term correlation coefficient, and g (when g is determined via a grid search) are the true values of those parameters, rather than estimates. The reported t -statistics may therefore be somewhat inflated.

¹⁶Estimates of the remaining parameters have been excluded to conserve space. There were no important differences between group and aggregate regressions relating to these parameters.

¹⁷Several other points could be made regarding the size of the d estimates. First, collinearity problems are likely in a model as complex as this one, and there is evidence that this may partially explain some unusually large values for d . The variance-covariance matrix of the estimated coefficients typically shows that the correlation between estimates of d and α_1 are large and negative, typically on the order of -0.85 , so collinearity is a possible problem. Second, some of the naive voter

Table 1. Aggregate Popularity Functions

	Sophisticated	Naive	Sophisticated (Corrected)	Naive (Corrected)
α_0	-0.024 (-1.492) ^a	-0.314 (-2.774)*	-0.025 (-1.205)	0.044 (.186)
α_1	-0.528 (-2.900)*	— —	-0.387 (-2.127)*	— —
$\dot{P}$	— —	-5.36×10^{-3} (-2.909)*	— —	-8.12×10^{-3} (-2.446)*
Q/QN	— —	.298 (2.558)*	— —	-0.061 (-.254)
P1	0.078 (2.007)*	0.114 (2.820)*	0.074 (1.649)*	0.075 (1.545)
P2	0.071 (2.000)*	0.113 (3.004)*	0.064 (1.593)	0.072 (1.611)
P3	0.028 (.776)	0.062 (1.616)	0.020 (.526)	0.027 (.637)
P4	0.043 (1.392)	0.066 (2.055)*	0.039 (1.169)	0.039 (1.077)
P5	0.048 (1.615)	0.079 (2.566)*	0.043 (1.406)	0.052 (1.575)
P6	0.002 (.065)	0.035 (1.190)*	-0.003 (-.111)	0.009 (.327)
Watergate	-0.152 (-3.873)*	-0.166 (-4.007)*	-0.137 (-3.110)*	-0.130 (-2.825)*
Killed in Vietnam	-1.56×10^{-5} (-1.732)*	-3.66×10^{-5} (-4.604)*	-1.60×10^{-5} (-1.577)	-2.29×10^{-5} (-2.185)*
Intercept dummies				
Eisenhower	0.823 (11.396)*	0.778 (9.544)*	0.831 (8.823)*	0.792 (7.823)*
Kennedy	0.758 (15.819)*	0.732 (13.713)*	0.753 (12.412)*	0.731 (11.223)*
Johnson	0.804 (10.756)*	0.697 (7.911)*	0.812 (8.371)*	0.764 (7.183)*
Nixon	0.744 (14.306)*	0.678 (12.290)*	0.745 (11.270)*	0.727 (10.072)*
Ford	0.796 (7.706)*	0.783 (7.966)*	0.842 (6.723)*	0.857 (7.018)*
Carter	0.638 (11.344)*	0.615 (11.656)*	0.658 (9.476)*	0.665 (9.496)*
g^b	0.870 —	0.518 (2.617)*	0.850 —	0.835 (1.278)
d	-0.013 (-2.324)*	— —	-0.019 (-1.591)	— —
SSR	0.218	0.231	0.188	0.196
R^2	0.850	0.841	—	—
D.W.	1.210	1.230	—	—
ρ	—	—	0.395	0.385

*Significant at .10 level or better.

^aAsymptotic *t*-statistics are in parentheses.^bTest of $H_0: g = 1$.

Table 2. Disaggregated Group Popularity Functions

	White Collar	Blue Collar	Non Labor Force	Republican	Democratic	Independent	College	High School	Grade School
Sophisticated voter models									
α_0	-0.031 (-1.103) ^a	-0.033 (-1.286)	-0.015 (-.511)	6.74×10^{-3} (.216)	-0.036 (-1.645)*	-0.025 (-.904)	-8.48×10^{-3} (-.180)	0.013 (.287)	0.037 (.716)
α_1	-0.694 (-3.536)*	-0.672 (-3.635)*	-0.422 (-2.376)*	-0.494 (-3.566)*	-0.644 (-3.714)*	-0.376 (-3.039)*	-0.452 (-1.303)	-0.392 (-1.187)	-0.348 (-.900)
d	-8.69×10^{-3} (-3.150)*	-9.90×10^{-3} (-2.471)*	-0.015 (-1.612)	-0.011 —	-0.010 (-2.338)*	-9.95×10^{-3} (-1.948)*	-0.020 (-1.112)	-0.023 (-1.052)	-0.032 (-.841)
g^b	0.700 —	0.770 —	0.690 —	0.660 —	0.790 —	0.720 —	0.790 —	0.800 —	0.820 —
SSR	0.187	0.152	0.127	0.210	0.127	0.197	0.158	0.140	0.182
ρ	0.305	0.290	0.230	0.335	0.160	0.210	0.450	0.450	0.440
Naive voter models									
α_0	0.708 (2.800)*	0.720 (2.350)*	0.654 (2.142)*	0.815 (3.769)*	0.669 (2.075)*	0.590 (2.163)*	0.290 (.638)	0.335 (.711)	0.354 (.608)
$\dot{p}$	-6.27×10^{-3} (-2.279)*	-7.64×10^{-3} (-2.357)*	-4.49×10^{-3} (-1.372)	-3.65×10^{-3} (-1.165)	-8.14×10^{-3} (-2.597)*	-7.42×10^{-3} (-2.500)*	-0.011 (-1.779)*	-9.68×10^{-3} (-1.528)	-0.012 (-1.484)
Q/QN	-0.721 (-2.871)*	-0.736 (-2.429)*	-0.665 (-2.219)*	-0.800 (-3.783)*	-0.690 (-2.158)*	-0.593 (-2.193)*	-0.271 (-.631)	-0.310 (-.693)	-0.301 (-.545)
g^b	0.736 (2.390)*	0.828 (2.011)*	0.795 (1.599)	0.740 —	0.854 (1.861)*	0.744 (2.169)*	0.786 (1.295)	0.820 (1.084)	0.851 (.977)
SSR	0.175	0.147	0.124	0.186	0.128	0.181	0.149	0.140	0.183
ρ	0.255	0.270	0.245	0.330	0.130	0.190	0.495	0.455	0.445

*Significant at .10 level or better.
a Asymptotic *t*-statistics are in parentheses.
b Test of $H_0: g = 1$.

The predicted consequences of following sub-optimal policies are reasonable in magnitude. Estimates from column 3 in Table 1 imply that if an administration were to deviate consistently from the optimal value of Q/QN by .05 over a four-year period, its rating would be 7.7 points lower than if it had followed optimal policies.

Results of the naive model regressions reported in columns 2 and 4 are somewhat weaker. In the uncorrected equation for the aggregate data in Table 1, signs on the output and inflation variables are properly signed and significant, but the sum of squared residuals is larger than for the corresponding sophisticated voter model. In the equation corrected for serial correlation, the inflation variable is again properly signed and significant, but the output variable has a surprising negative sign and is not significant. Results from the group equations are even more troubling. In those equations inflation generally has the anticipated negative effect on popularity; however, the output variable is always incorrectly signed, often significantly so. Although sums of squared residuals (corrected for the error process) are lower in some naive equations, this is largely attributable to the implausible negative coefficients on the output variable. When output coefficients are constrained to be non-negative, sums of squared residuals are lower for sophisticated voter models than for naive voter models in 9 of 10 comparisons, although differences are not typically large.

Noneconomic variables perform much as expected in all models. In the sophisticated voter models, the parameter α_0 is usually negative, but not significant. The negative sign implies that even if a president followed optimal policies, there would be a downward trend in his popularity over the course of his stay in office. The literature provides several possible explanations for such secular trends,¹⁸ and most empirical investigations have found evidence of them. In the naive voter model, the interpretation of a_0 differs,¹⁹ but for

models in the literature also indicate extreme inflation aversion, so the findings here are consistent with previous ones. See for example Hibbs (1982a), who finds that Republicans have a marginal rate of substitution of 4.3 points of unemployment per point of inflation. This would indicate that Republican voters would be willing to endure 43% unemployment in order to eliminate a 10% inflation. Finally, we find that the overall performance of the model is strong even if we constrain the inflation aversion parameter to assume smaller and more theoretically plausible values.

¹⁸See Mueller (1970, 1973) and Stimson (1976).

¹⁹This is because N_t , as defined in equation (6), does not take on a value of zero at an optimal policy choice. Indeed, the naive model has no well-defined optimal policy choice, and what it regards as good policies might not be sustainable over the course of a term in office.

most plausible values of the economic variables that model too usually implies secularly declining popularity. Table 1 shows that the series of early term dummies pick up honeymoon effects which are often significant, but which appear to be exhausted by the sixth quarter in office. The Vietnam war variable and the Watergate dummy are negatively signed as expected. The honeymoon, Vietnam, and Watergate variables have consistently similar effects in the group regressions.²⁰ The memory parameter g takes on values ranging from .66 to .85 in the various models, which indicates some discounting of the past, and most magnitudes are roughly similar to those found by Hibbs (1982a, b, c).

None of the results suggests striking behavioral differences among diverse groups. Whether this indicates the absence of group-related differences in preferences or simply the inability of the empirical analysis to make such fine distinctions remains questionable.²¹

We have argued that our political support function is a sensible one; however, a number of its features reflect choices for which there exist reasonable alternatives. In order to assess the robustness of our major findings concerning the sophisticated and naive standards of performance, we have estimated additional equations in which specific features of the model are modified, or alternative data sources are employed. Because of the substantial computing requirements, we have used exclusively the aggregate popularity data in these sensitivity tests. The modified models make the adjustments noted below.

1. Alternative formulations are used for $f(T^*)$.
2. Weighted averages of past values of "killed in Vietnam" are used instead of current values.
3. Partial adjustment in the control of output is assumed to reflect imperfect control of policymakers over the resulting outcomes.
4. Nominal rather than real output is assumed to be the control variable for policymakers.
5. Alternative data replace the estimates of natural rates of output in the model.
6. The models are estimated for subperiods of the 1957-1980 time span.
7. Alternative formulations of the Watergate effect are specified.

²⁰The naive models do consistently indicate a larger effect of killed in Vietnam than do sophisticated voter models. This could reflect collinearity between the war and excessively expansionary economic policies in the sophisticated model.

²¹Our results here differ from those of Hibbs (1982a), who concluded that groups' "objective" economic interests were reflected in his estimates of marginal rates of substitution of inflation for unemployment.

8. A quadratic performance measure is used in the sophisticated voter model.

Complete explanations and detailed results of the sensitivity tests are provided in the Appendix.

Results of the sensitivity tests generally reinforce our earlier conclusion that the sophisticated voter hypothesis performs well. In all of the modified sophisticated voter models, the results indicate that suboptimal performance is punished, and the crucial economic parameter α_1 is usually statistically significant. In some models, estimated inflation aversion is quite strong, but the relevant parameter d is never significantly different from plausible values. None of the modified models substantially outperforms the model presented in the text; all of the sophisticated voter models perform comparably well. The one result that was most damaging to the sophisticated voter hypothesis was that although the sophisticated voter model outperformed the naive for the 1957 to 1968 subperiod, the reverse was the case for 1969 to 1980. Thus, the sensitivity testing does not suggest a single superior model, but does basically confirm the robustness of earlier conclusions.

Conclusions

Two central themes emerge from our analysis. First, given the nature of the constraints on possible macroeconomic outcomes, sensible evaluation of economic performance is a little more complicated than is usually recognized in the study of political support. The way that most support models assume that voters evaluate economic performance implies naivete and vulnerability to manipulation. We have conceptualized and presented an alternative model which assumes that voters are more sophisticated.

Second, in empirical tests, the sophisticated voter model performs well by normal standards. Indeed, with very few exceptions it performs consistently better than the more conventional model, which implicitly assumes naivete.

These two themes are independent. The first set of observations would stand in the absence of empirical support for the sophisticated voter model, and although we find the empirical work remarkably supportive of the hypothesis that voters are sophisticated, that work should be viewed as establishing the credibility of an alternative view, rather than as providing conclusive support for it.

We cannot, of course, say anything directly about the cognitive processes of the voter, but whatever those processes involve, we can say on the basis of our evidence that popular support of incumbent presidents is consistent with a sophisticated standard of evaluation.

The consistently good performance of the

sophisticated voter model does illuminate sometimes puzzling features of the existing literature on politics and macroeconomics. For example, several studies of political support have reported unstable coefficients on unemployment and on income growth, often with the wrong or at least the unexpected sign, given the typical assumption that unemployment is always punished and growth is always rewarded.²²

These findings are not so surprising when viewed with the central insight of the sophisticated voter model, that is, that since economic variables are empirically interdependent, the evaluation of any one of them may not be independent of the others. Specifically, although economic slack is in general undesirable, it may occasionally be accepted in times of high inflation in order to restore price stability.²³

Similarly, several studies have failed to find evidence of the political business cycles one would expect of politicians maximized a naive, retrospective voting function like that which typifies the literature.²⁴ MacRae (1977) found indirect evidence that some politicians might believe or assume that voters are strategic, but we provide direct evidence that voting behavior may be sophisticated. Political business cycles may not exist in part because voters do not offer an incentive for politicians to induce them.

Our findings speak also to the rationales for retrospective voting articulated by Key and Downs, and to related issues of rationality and accountability. Key defended a process in which voters were expected to do little more than vote against incumbents who had displeased them. He articulated no particular standard of rationality and said nothing about the ability to predict the future from past performance. "The vocabulary of the voice of the people consists mainly of the words 'yes' and 'no'; and at times one cannot be certain which word is being uttered" (1964, p. 544).

Downs expected more of voters than Key, including an ability to use the past to predict the

²²See, for example, Arcelus and Meltzer (1975), Ken-ski (1977), Powell (1982), and Stigler (1977).

²³Ironically, what may have been taken as an indicator of irrationality on the part of voters may be evidence that voters follow a higher standard of rationality than the designers of the studies of voting behavior. This would have delighted E. E. Schattschneider, who asked, "Who are these self-appointed censors who assume that they are in a position to flunk the whole human race?" He also observed that "political research is never better than the theory of politics on which it is based" (1960, pp. 135, 133).

²⁴See, for example, Beck (1982), Golden and Poterba (1980), and Monroe (1983).

future, but he seems not to have expected more than a simple projection of past to future performance. He did not anticipate that such a projection might not be a good predictor.

The political business cycle literature and an understanding of macroeconomic dynamics tends to undermine both Key and Downs by suggesting that retrospective voting can be a poor predictor of the future and can make voters vulnerable to manipulation. On the same grounds, evidence that voters reward and punish incumbents on the basis of simple retrospective measures of recent economic performance should not be taken to indicate the collective rationality of the electorate or the accountability of public officials.²³

But models representing voters as more sophisticated and less vulnerable to manipulation have not been rejected; they have not previously been tried. Our evidence that voters may not be naive suggests that Key and Downs are not so much undermined as in need of minor refinement. In some respects, our analysis brings the two perspectives together into a more comprehensive view of retrospective voting.

Whether or not retrospection is meant to predict the future or merely to hold incumbents accountable for the past, the standard of evaluation used is important. Insofar as retrospection is based on reasonable standards, it makes little difference whether the main interest is backward looking or forward looking. The incentives will be the same for the politician; good performance in term one will be compatible with good performance in term two and so on. When we modify Key and Downs to rule out simple standards which are misleading about the future, the difference between the two views is not very great. But without such modification in the context of macroeconomic issues, neither one provides a fully satisfactory basis for a theory of voting that would make public officials accountable for their performance.

Appendix

This appendix provides detailed descriptions of sensitivity tests in which modified versions of our popularity functions have been estimated using aggregate data. For each test we briefly describe the rationale for the modifications made and report the relevant results.

1) We have used alternative formulations for $f(T_i^*)$, which attaches weights to cumulative economic performance at different points in time. The model in the text assumes that $f(T_i^*) = \sqrt{T_i^*}$.

This function increases as T_i^* increases, but does so at a decreasing rate. A number of other functions also exhibit these properties, however. In sensitivity testing we have generalized by letting $f(T_i^*) = (T_i^*)^\omega$ and have used various values of ω between 0 and 1. The major conclusions noted in the text hold up when the model is generalized in this manner. The lowest sum of squared residuals for the sophisticated and naive voter models is obtained when $\omega = 1$, that is, when $f(T_i^*) = T_i^*$, so that the weight attached to performance increases linearly with time in office. Results for these regressions are provided in Table A1. The sophisticated voter model has a lower sum of squared residuals than the naive voter model for all values of ω for which we estimated equations.

2) In our original model, only current values of number killed in Vietnam were assumed to affect popularity. Because voters may also remember and attach importance to past values of this variable, we have also estimated models in which weighted averages of past values affect popularity. The parameter g determines weights used to compute the average for killed in Vietnam, as well as for the economic variables. As Table A2 shows, this change resulted in a smaller coefficient for killed, but slightly increased the sum of squared residuals relative to the original specification of the sophisticated voter model.

3) Like most previous models, our model assumes that policymakers directly control real output. In reality that control is indirect, operating through monetary and fiscal policies, and it is imperfect because of exogenous shocks, lags in

Table A1. $\omega = 1$

	Sophisticated	Naive
α_0	-2.31×10^{-3} (-.789)	-3.44×10^{-3} (-.092)
α_1	-0.070 (-2.170)*	- -
$\dot{P}$	- -	-1.52×10^{-3} (-2.617)*
Q/QN	- -	1.67×10^{-3} (.045)
g^a	0.820 -	0.772 (1.663)*
d	-0.020 (-1.850)*	- -
SSR	0.184	0.190
ρ	0.425	0.425

*Significant at .10 level or better.

^aAsymptotic t -statistics are in parentheses.

^bTest of $H_0: g = 1$.

²³For examples of such inferences, see Tufte (1975, p. 826) and Powell (1982, pp. 19-26).

Table A2. Weighted Averages of Killed in Vietnam Included

	Sophisticated	Naive
α_0	-0.018 (-.902) ^a	-0.301 (-2.128)*
α_1	-0.446 (-2.059)*	-
$\dot{P}$	-	-5.42x10 ⁻³ (-2.377)*
Q/QN	-	0.290 (1.992)*
g^b	0.850 -	0.606 (2.124)*
Killed in Vietnam	-1.85x10 ⁻⁶ (-.400)	-7.03x10 ⁻⁶ (-2.922)*
d	-0.017 (-1.787)*	-
SSR	0.194	0.195
ρ	0.385	0.395

*Significant at .10 level or better.
^aAsymptotic *t*-statistics are in parentheses.
^bTest of $H_0: g = 1$.

implementation, and lags in effectiveness. We have investigated a modification of the model in which voters recognize that there is "inertia" to be overcome in adjusting Q_t and that there might be substantial costs associated with overcoming that inertia too quickly. Specifically, we build a partial adjustment process into the optimal feedback rule used to define desirable policies.

As before, let Z_t^* refer to that value of Z_t judged best without any consideration of the costs of overcoming inertia. As earlier noted, $Z_t^* = d\dot{P}_{t-1}$. Now, let Z_t^{**} refer to a "second-best" value of Z_t , that is, the policy judged to be best given the existence of inertia in policymaking. We assume that Z_t^{**} is defined by:

$$Z_t^{**} - Z_{t-1} = \phi(Z_t^* - Z_{t-1})$$

where $0 \leq \phi \leq 1$. This equation says that in a given period we should move Z toward its no-inertia optimum Z^* , but because of the costs of changing Z rapidly, we should move it only part of the way. The parameter ϕ indicates how closely the second-best optimum approaches the no-inertia optimum. Measuring performance as the absolute value of the deviation of optimal from actual performance:

$$S = |Z_t - Z_t^{**}|$$
$$= |Z_t - (1 - \phi) Z_{t-1} - \phi \dot{P}_{t-1}|$$

This alternative measure of S_t is substituted into the popularity function. Possibly because of collinearity problems resulting from the added complexity of this model, unconstrained estimates of d and ϕ were not economically meaningful. However, when plausible values for d and ϕ were imposed α_1 was negative and highly significant, indicating punishment for suboptimality. Table A3 provides results assuming $\phi = .8$ and $d = -.010$.

4) As an alternative modification of the assumption that Q is a control variable, we estimate a model where nominal output is assumed to be the policy instrument. In practice, policymakers have considerable ability to control the growth of nominal output, but much less control over how that growth is divided between changes in output and changes in prices.

For this model, we change our measure of policy stance Z to the following:

$$Z_t = (NOMQ_t/EXNOMQN_t) - 1$$

where $NOMQ$ is nominal GNP and $EXNOMQN$ is the value nominal GNP would take if Q were equal to QN and if the inflation rate were to continue to follow current trends.²⁶ If policymakers were to set $NOMQ$ equal to $EXNOMQN$, this would be consistent with achieving real output of QN and not altering the inflation rate. Higher values of $NOMQ$ would be expansionary, whereas lower values of $NOMQ$ would be contractionary.

Estimates of this model are provided in Table A4. All results are consistent with hypotheses and

²⁶Trend rates of inflation are computed as predicted values from a regression of inflation on its last four lagged values.

Table A3. Partial Adjustment Model

α_0	-0.026 (-1.249) ^a
α_1	-0.759 (-3.572)*
ϕ	0.800 -
g^b	0.890 -
d	-0.010 -
SSR	0.194
ρ	0.390

*Significant at .10 level or better.
^aAsymptotic *t*-statistics are in parentheses.
^bTest of $H_0: g = 1$.

are very similar to these obtained with the original model.

Table A4. Nominal Q as a Control Variable

α_0	-0.024 (-1.169) ^a
α_1	-0.379 (-2.190)*
g^b	0.850 —
d	-0.019 (-1.660)*
SSR	0.187
ρ	0.400

*Significant at .10 level or better.

^aAsymptotic t -statistics are in parentheses.

^bTest of $H_0: g = 1$.

5) We have used alternative data sources to construct measures of policy stance. These alternative runs permit us to determine if the results are sensitive to the use of particular sources of data. In place of Gordon's natural rates of GNP, we have substituted estimates of "high-employment" GNP computed by Tatom (1982). We also estimate models in which the difference between

unemployment and its natural rate²⁷ replaces $Q/QN - 1$ as the measure of policy stance. Using Tatom's measures of high-employment GNP in place of QN , the model provides a slightly better fit than the model in the text, and all results are qualitatively similar. Table A5 shows that the inflation aversion parameter is slightly lower ($d = -.016$) than that in Table 1, and the estimate of α_1 remains negative and is more highly significant than in Table 1.

Using $U - UN$ in place of Z to measure policy stance also slightly improved the overall fit and led to results consistent with hypotheses. (Note that the hypothesized sign for d is positive in this model.) These results are also provided in Table A5 as are estimates of a naive model in which the unemployment rate replaces Q/QN as a measure of stimulus. In that model the unemployment variable was incorrectly signed and the sum of squared residuals remained slightly higher than for the sophisticated voter model.

6) We have estimated sophisticated and naive voter models for sub-samples of the period from 1957 to 1980. This permits us to see if the models perform consistently well in different periods and may also show whether there are perceptible changes in respondent preferences in periods

²⁷The natural rates of unemployment are also obtained from the mimeographed supplement to Gordon (1981).

Table A5. Alternative Data

	Potential GNP replaces QN	$Z = U - UN$	Naive with Unemployment
α_0	-0.022 (-1.128) ^a	-0.029 (-1.477)	-0.032 (-.781)
α_1	-0.605 (-2.737)*	-7.80×10^{-3} (-2.068)*	— —
$\dot{P}$	—	—	-8.19×10^{-3} (-2.271)*
U	—	—	2.85×10^{-3} (.448)
g	0.830 —	0.810 —	0.853 (1.111)
d	-0.016 (-3.364)*	0.848 (1.897)*	— —
SSR	0.179	0.186	0.194
ρ	0.365	0.370	0.425

*Significant at .10 level or better.

^aAsymptotic t -statistics are in parentheses.

^bTest of $H_0: g = 1$.

Table A6. Subsample Estimates

	Sophisticated			Naive	
	1957-68	1969-80	1969-80 (d constrained)	1957-68	1969-80
α_0	-0.059 (-2.307)* ^a	-0.031 (-.895)	-0.013 (-.318)	4.950 (3.207)*	-0.329 (-1.098)
α_1	-0.711 (-3.719)*	-0.290 (-1.434)	-0.366 (-2.671)*	-	-
g^b	0.850	0.410	0.750	1.089 (5.517)*	0.655 (-1.404)
$\dot{P}$	-	-	-	0.028 (.758)	-8.51×10 ⁻³ (-1.533)
Q/QN	-	-	-	-5.112 (-3.197)*	0.334 (1.187)
d	-0.010 -	0.019 (1.222)	-0.010 -	- -	- -
SSR	0.064	0.083	0.096	0.059	0.084
ρ	0.435	0.290	0.345	0.565	0.385

*Significant at .10 level or better.

^aAsymptotic *t*-statistics are in parentheses.

^bTest of $H_0: g = 1$.

where economic conditions differed considerably.

The sophisticated voter model performs as hypothesized for the 1957-1968 sample, but the inflation aversion parameter was incorrectly signed in the 1969-1980 equation. However when d was constrained to take a theoretically plausible value ($d = -.010$) in the latter sample period, then the estimate of α_1 was negative, as hypothesized, and significant. In contrast, the naive model performs poorly for the 1957-1968 period, with both the inflation and unemployment variables incorrectly signed. However, for the latter period the signs are reversed, and the sum of squared residuals is lower than for the sophisticated model. These results are provided in Table A6.

7) We have used alternative measures for the Watergate variable. These measures varied both the length of the effect of Watergate and the possibility that a trend (rather than a dummy) would better capture the cumulative effects of the Watergate revelations. Results are shown in Table A7, in which the Watergate effect is defined as a trend beginning with Nixon's second term and continuing until he left office. All of the substantive results were unchanged regardless of how we defined the Watergate variable, although the results in Table A7 do provide a slightly lower sum of squared residuals.

8) As a performance measure we have used the squared deviations of actual from "optimal"

policies instead of the absolute values of those deviations:

$$S_t = (Z_t - Z_t^*)^2.$$

In an earlier version of this article we used this quadratic performance measure throughout and

Table A7. Alternative Watergate Variable Specification

	Sophisticated	Naive
α_0	-0.015 (-0.681)	0.083 (.324)
α_1	-0.357 (-1.855)*	-
d	-0.019 (-1.525)	-
g^b	0.860 -	0.852 (1.099)
Watergate trend	-0.037 (-3.655)*	-0.035 (-3.467)*
SSR	0.178	0.187
ρ	0.450	0.380

*Significant at .10 level or better.

^aAsymptotic *t*-statistics are in parentheses.

^bTest of $H_0: g = 1$.

Table A8. Quadratic Performance Measure

α_0	-0.029 (-1.451) ^a
α_1	-0.838 (-1.837)
g^b	0.810
d	-0.039 (-1.676)*
SSR	0.184
ρ	0.425

Significant at .10 level or better.

^aAsymptotic t-statistics are in parentheses.

^bTest of $H_0: g = 1$.

obtained results quite similar to those presented here. In Table A8 we report estimates for the sophisticated model based on this performance measure. Although the estimate of d is especially large and α_1 is not significant, the parameters are properly signed.

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A Reevaluation of Realignments in American Politics: Evidence from the House of Representatives

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This article reevaluates American realignment literature based on Clubb, Flanigan, and Zingale's (1980) admonition to focus on control of government and political leadership rather than electoral results. I put forward a theory of policy change in the House of Representatives which shows, like Sinclair (1977), that the effect of electoral realignments is to create a strong and unified majority party in the Congress. However, unlike other work that focuses on electoral courses, I show that structural features of elections created the new majority party in both the Civil War and the 1890s realignments. Specifically, I argue that in these two realignments a strong regional seats-to-votes distortion created the Republican majorities that enacted the policy changes associated with these realignments.

The structural arrangements of the American political system were designed to limit any majority's ability to legislate major policy shifts. Federalism, separation of powers, and checks and balances are impediments to decisive majority decisions. Dahl (1956), Orfield (1975), and Huntington (1965), among others, have discussed the effects of these structural arrangements on policy-making in the U.S. Congress. In an attempt to show how these cumbersome governmental structures have at times yielded significant policy changes, a full generation of political scientists and historians has been schooled in the notion that realigning elections are the mechanisms that account for major policy shifts. Those who study realignments have claimed much. V. O. Key, Jr. (1955) wrote that in realignments "more or less profound readjustments occur in the relations of power within the community." Key's student, Walter Burnham (1970), refers to critical elections as "the mainsprings" of American politics. The terminology of the realignment perspective has also been adopted by traditional historians and by journalists such as David Broder to the extent that American political history is divided in terms of realignment eras, which makes realignment theory a basic organizing framework for the study of American history.

One of the major reasons for the popularity of the realignment perspective is that it links electoral behavior to institutions and to major shifts in public policy. As developed by various scholars, the basic realignment scenario holds that societal tensions in the form of cross-cutting issues, such as slavery in the 1850s, emerge to divide the country (Beck, 1979; Burnham, 1970; Ginsberg, 1976; Sundquist, 1973). The result of these issues is that the two major parties take opposing positions on the issues of the realignment (Ginsberg, 1972, 1976; Sundquist, 1973) and in the realigning election or elections, the voters put into place "new and durable electoral groupings" (Key, 1955). The newly elected majority party then enacts the "cluster of policy changes" associated with the realignment (Burnham, 1970; Ginsberg, 1976; Sinclair, 1977). Although the extent to which realignments are the mainsprings of policy changes in the American policy process is still in question (Brady, 1978; Converse, 1974; Rusk, 1974), it seems clear that this perspective serves to link elections, institutions, and policy and to provide greater intellectual order to the varied panorama of American history.

Given that there are three units of analysis in the realignment perspective—elections, institutions, and policy results—it is somewhat surprising that most work has been concentrated on studies of realignment in the electorate and little has been done on the effect of realignment on the governmental leaders who shape and push through the clusters of policy changes associated with them. In fact, Clubb, Flanigan, and Zingale (1980, p. 16) attempted to correct this by seeking "to redirect attention away from the role of electoral change and toward the control of government and the role of political leadership in understanding the phenomenon of partisan realign-

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ment." There are some studies of the effects of critical elections on the House of Representatives and the policy process (Brady, 1978, 1980, 1982; Sinclair, 1977), but much remains to be done before the linkages between realigning elections and policy change through institutions will be well understood. Nevertheless, the central concept of realigning elections is important for scholars concerned about mass and elite behavior in democratic countries.

In this article I examine the three major American realignments—the Civil War, the 1890s, and the New Deal. The general thesis is that in each of the realignments the effect of the realigning elections was to create a unified majority party capable of enacting major policy shifts. However, unlike other studies of realignments, the emphasis is on some important electoral differences between these realignments. Specifically, I shall argue that the Civil War and the 1890s realignments were more the result of structural factors than of massive electoral shifts, in contradistinction to the dominant view that the policy changes are the result of changes in mass voting behavior (Burnham, 1970). I begin by briefly reviewing the major factors that deter the ability of the House to legislate major policy changes. In light of these factors, I put forward a theory of policy change in the House and then test the theory over the Civil War, 1890s, and 1930s realignments.

Major policy changes occur rarely in the U.S. House of Representatives for a number of reasons. The most important of these are: 1) members are normally elected by local interests on local issues (Mann, 1978; Mann & Wolfinger, 1980); 2) once elected, members choose, within limits, committee assignments based on those local interests and issues, thus localizing rather than nationalizing policy alternatives (Cooper, 1970; Shepsle, 1978); 3) the Committee system is powerful in part because it is stable (Anderson, 1979; Fenno, 1966, 1973; Goodwin, 1970; Huitt, 1954, 1957); 4) the congressional parties are normally weak and divided; thus they cannot put together coalitions that can override localism (Orfield, 1975); 5) the organization, rules, and procedures of the House facilitate the interests of those who wish to preserve the status quo (Cooper, 1977; Shepsle, 1979). It should also be mentioned that there are many historical periods where inertia or incrementalism are in accord with both the majority of the public and the majority of the House (Jones, 1970). When this is the case, the congressional system is in harmony with political pressures. However, as Burnham (1970) has argued, political systems must over time adjust to majority pressures for change. It is from this perspective that we look to elections to see how the U.S. House of Representatives has at

times overcome these powerful biases toward incrementalism to produce significant policy changes. How do elections generate changes that allow the House to produce major policy shifts?

A Theory of Policy Change

It is obvious that elections do not generate changes in the basic structures of federalism, separation of powers, checks and balances, and single member plurality elections. These structural characteristics have survived many elections. The answer to the question posed above is that certain critical elections reflect partisan realignments that alter radically the standard policymaking process. To understand how these realigning elections affect major public policy changes, it is necessary to begin with an understanding of the generally localized nature of congressional elections. House elections normally are determined by local factors (local interests, organization, and issues), thereby assuring the dominance of localism in politics in the House. In each American realignment, elections are dominated by national issues. Before realignments, cross-cutting issues arise which do not fit within the framework of the existing two-party system. Ultimately, the parties take positions on the issues that offer clear-cut alternatives to voters. (In the case of the Civil War realignment, the Republicans replaced the Whigs before the choice was clear.) When the realigning election or elections occur, the result is a new congressional majority party elected on its positions on national issues, not local issues. Moreover, the new majority party maintains uninterrupted control of the presidency and both branches of Congress for over a decade (Clubb, Flanigan, & Zingale, 1980).

The effect of these elections in the House is that they reduce the major drawbacks to party strength in the House. Elections that are decided on a localized basis make the congressional parties amalgams of differing interests, for example, southern rural and northern urban Democrats during the 1940s through 1970s. Under normal electoral conditions, congressional parties are not united across a whole series of issues. In recent times, Democrats disagreed over civil rights, social welfare, and Vietnam policies, among others, and as long as elections are decided on the basis of local factors, policy differences are accommodated and incrementalism results. During realignments, representatives are elected more on the basis of party positions on the national issues than on local issues. Thus, the majority party is relatively united on the issues of the realignment. Local factors do not radically compromise policy choices, and a unified majority party votes in major policy changes. The argument is not that

local factors are totally overridden, but rather that the balance between local and national factors shifts toward national forces (Sorauf, 1968).

A second drawback to party strength in the Congress is the committee system. Committees have lives of their own, and the more important the committee, the more stable its membership. The stable leadership passes along committee norms and strategies to new members, and, thus policy in the committee's area does not change greatly. The influx of new members during realignments eliminates committee stability, and new leaders support the party position for major policy innovations. In sum, the influx of new members during realignments reduces the impact of localism and committee stability in the House (Brady, 1978, 1980). The result is increased party structuring of voting, especially on the issues of the realignment.

If this notion of realignments is correct, I should be able to show that in each of the three major realignments—Civil War, 1890s, and 1930s—the following occurred.

- 1) The dominant parties took polarized positions on the cross-cutting issues.
- 2) The election results were more national, less local.
- 3) The "new" majority party controlled the government for at least a decade.
- 4) Committee turnover was high.
- 5) Party structured voting on the realignment issues.
- 6) Significant policy changes occurred during these eras; especially relevant are nonincremental changes in appropriations.

The Theory Tested over Three Realignments

In each of the major realignments, a major cross-cutting issue dominated the election. The second American party system was broken up by the rise of the slavery and free labor issues. The two dominant parties of the 1832-1852 period were the Whigs and the Democrats, both of which had northern and southern wings. The compromise of 1850 plus the Democrats' acceptance of federal activity to expand the economy diminished the differences between the parties such that by 1853, the Whigs were no longer a viable electoral force (Holt, 1978, chaps. 5 and 6). Senator Douglas's introduction of the Kansas-Nebraska Act "permanently severed the Northern and Southern wings of the Whig party and eventually obliterated what remained of it in the South" (Holt, 1978, p. 148). In the elections of 1854 and 1856, the Whigs were replaced by Republicans in the Middle West and Know-Nothings in the East. The nativism issue faded in the East, and by 1858

the Republicans were clearly the Democrats' major competition. In short, by the late 1850s the electorate was offered a clear choice between parties and policy on both slavery and free labor.

The cross-cutting issue of the 1890s realignment was the issue of "what future for America—industrial or agricultural?" The rise of industrialization in the aftermath of the Civil War generated the displacement of farmers and a change from a society of local, self-sufficient communities to a highly interrelated industrial society (Schattschneider, 1960). The result was both the displacement of agricultural interests and the end of a way of life. The overarching issues were specifically focused on the questions of gold, silver, the protective tariff, and American expansionism. Because they were debtors, agricultural interests favored the inflationary coinage of silver at a 16-to-1 ratio to gold, free tariffs and anti-expansionism. Industrial interests were opposed to these policies. The rise of the pro-farmer, Populist party is testimony for the existence of cross-cutting issues in the 1896 realignment. Under William Jennings Bryan, the Democrats adopted the Populist position, while the Republicans adopted a pro-position on gold, protective tariffs, and expansionism.

The cross-cutting issue during the New Deal realignment was the result of a single event—the Great Depression—and the question was whether the government would adopt policies to combat the effects of the Depression. The Republican incumbent, Herbert Hoover, would not adopt policies to aid farmers, workers, cities, and the unemployed. By 1932 the Democratic party had answered by proposing relief funds and programs to aid those most affected by the Depression. Once again voters were offered clear-cut choices between parties and policy.

In order to demonstrate the polarization of opinion associated with realignments, in Table 1 I summarize Ginsberg's (1972, 1976) content analysis of party platforms and show the party differences on these issues in each of the realignments.

Table 1 clearly shows that the Civil War and the 1890s realignment were characterized by deepening party differences in regard to the cross-cutting issues leading up to the critical elections. The pattern for the New Deal realignment is somewhat different. The parties differed in 1924, but it was not until 1932 that the Democrats could convince the electorate to send a new majority party to Congress. In addition, the magnitude of these figures suggests that the parties were less polarized in the positions they took during the New Deal than they were during the Civil War and 1890s realignments (also see Brady & Stewart, 1982). Still, the first part of the theory holds: in each of

Table 1. Partisan Platform Differences on Major Issues in Three Realignment Eras^a

	Slavery	Capitalism	Depression
Civil War			
1848	0		
1852	.10		
1856	.24		
1860	.71		
1890s			
1884		.08	
1888		.04	
1892		.44	
1896		.55	
New Deal			
1920			.02
1924			.30
1928			.19
1932			.26

^aAdapted from Ginsberg (1972, p. 612). Higher values represent greater disagreement.

the realignment eras, the major political parties took opposing positions on the issues of major concern. Thus, when voters went to the polls in the elections of 1860, 1896, and 1932, they were offered "a choice, not an echo."

National, Regional and Local Effects on Voting

If realigning elections are precipitated by cross-cutting national issues, then the congressional election results should demonstrate national or regional rather than local electoral forces at work. The standard technique used to determine national rather than local factors is to calculate the variance across House election results over the relevant time period. The argument first put forward by Butler and Stokes (1969) is that when variance around the mean is low, national factors are at work, and, conversely, when variance is high, local factors are predominant. Butler and Stokes used this argument to show how national factors are more important in Britain than they are in the United States. Their point was that variance around the mean swing in pairs of elections measures the uniformity of change, that is, the extent to which national forces are at work. However, testing for change over time within a single country presents some difficulties.

The first and foremost problem is that as Flanigan and Zingale (1974) have shown, realigning change can be either compensating or across the board. In a compensating realignment, both par-

ties gain some votes while losing others. Of course one party gains more than it loses and vice versa for the other party. In an across-the-board realignment, one party gains votes while the other party loses votes. In both cases, as one moves to the realigning election or elections, the variance around the mean is affected because vote totals in districts are shifting, and measures of variance are squared differences; thus, the direction of change is irrelevant. Determining whether a realignment is compensating or across the board is particularly important in the American case because of the regional and cultural variation across the American states (Elazar, 1972).

The strategy for analyzing electoral change in each of the three American realignments is first to show the mean swing in the vote and standard deviation for the following time periods: 1852-1876, 1884-1900, and 1924-1940. The measure of change to be used is the coefficient of variation, which is $V = S/X$, where S is the standard deviation and X is the mean. I want to take into account both the mean swing for or against a party and the variance around that mean. The argument is that as the standard deviation increases relative to the mean, local variation is greater than the national trend, whereas as the mean swing increases relative to the standard deviation, then national trends eclipse local factors. In short, the lower the value of the coefficient of variation, the greater the likelihood of an election reflecting national rather than local factors.

Upon completion of this analysis, I focus on regional variation in each of these realignments, and finally, I turn to the Flanigan and Zingale (1974) analysis-of-variance technique to determine the nature of the realignments.

Testing for national effects during the Civil War realignment era constitutes a problem in that the demise of the Whigs, the rise of the Republicans, the secession, and the later readmission of 11 southern states to the Union during this time period complicates analysis of electoral results. Ideally such a time series should be run on both major parties for all points in the set (Clubb, Flanigan, & Zingale, 1980). Obviously, this is not possible for the 1848-1876 period; thus the analysis for the Civil War period focuses on the Democrats' percentage of the vote in each national election. The Democrats contest elections throughout the era including the period of secession. Silbey's (1977) work demonstrates the party's strength throughout the era. During the Civil War proper, the Democrats almost control the House in the 1861-1862 election, and they only narrowly lost the presidency in 1864. Testing for national versus local factors is possible because the Democrats were both stable and competitive over the entire

time period.¹ The analysis of electoral changes for the other two realignments is straightforward, given that the Democrats and Republicans are stable in both eras. The data in Table 2 show the average swing in the vote and the average of the *V* coefficients for the pre-realignment, realignment, and post-realignment periods. The realignment elections for each period are: Civil War realignment, 1854-1860; 1890s realignment, 1894-1896; New Deal realignment, 1932-1936.² Table 2 also shows the mean swing and *V* coefficient for the single election that gave the new majority party control of the presidency, the House, and Senate —1860, 1896, 1932.

During the pre-Civil War realignment (1846 through 1852), the average swing was 3.26 and the average *V* coefficient was 8.09, which indicates a relatively low swing and considerable variation in voting patterns. In contrast, the realignment period (1854-1860) has a mean swing of -5.31 away from the Democrats and an average *V* coefficient of -4.34, indicating national electoral factors at work. The post-realignment period (1862-1876) has a mean swing of only 2.87 and a mean *V* coefficient of 8.15. These data support the hypothesis of increased nationalization of elec-

toral results during the Civil War realignment. The data for the 1860 election are even more dramatic, a swing vote of 6.07 away from the Democrats and a low *V* of 2.8.

Unlike either of the two other realignments, the Civil War era has major structural changes which affect the electoral results. The replacement of the Whig Party by the Republicans and the secession of the southern states and their readmission are the major structural changes. The elections of 1853-1854 and 1855-1856 are the two decisive elections that replace the Whigs as a major party. The analysis of these elections shows a dramatic increase in the mean swing relative to the variance—scores of -2.8 and 2.9, respectively. The Republicans actually won a plurality of House seats in the 34th House. The Democrats lost 7.41% of the vote, while variance dropped to 20.7. In the 35th House the Democrats gained from the mean swing of 6.34%, while variance dropped to 18.4. The period of the 33rd through 35th Houses realigned party preferences from Whig-Democrat to Republican-Democrat.

The election of 1859-1860, which brings the Republican party full control of the government (president, Senate, and House) also has a coefficient of variance of less than 3.0. In this election, the Democrats lost 6.07% of the vote, and the standard deviation is 17.6. The only other election pair with a coefficient of variance under 3.0 is the 40th-41st (1866-1868) pair. In this pair, the Democrats gained 3.8%, while the variance was only 8.4. The Democratic gain was, in part, a repudiation of radical Republicanism plus the strong Democratic returns from border states. In sum, the elections with low variation coefficients correspond to the structural changes mentioned above. Or as Clubb, Flanigan, and Zingale wrote:

¹The addition of the Republicans beginning in the 36th House does not change the substantive results.

²The following analyses include all election pairs in districts. I have not excluded races where little or no competition exists; thus when a candidate is unopposed in one race and an opponent garners 30% of the vote in the next, a major swing is recorded. Some scholars limit these calculations to competitive districts, thereby causing lower standard deviations. My approach was that in periods of major change, such data should not be thrown out.

Table 2. Aggregate Vote Swings and V Coefficients during the Civil War, 1890s and New Deal Realignments

	Pre-realignment	Realignment	Post-realignment	Controlling Elections
Civil war realignment	1846-1852	1854-1860	1862-1876	1860
X Dem	3.26	-5.31	2.87	-6.07
V	8.09	-4.34	8.15	-2.81
1890s realignment	1884-1892	1894-1896	1898-1902	1896
X Dem	-1.79	-5.45	-3.13	
V	-15.41	-3.05	6.27	
X Rep	2.62	4.39	-2.13	3.67
V	-8.21	3.20	5.46	3.90
New Deal Realignment	1924-1930	1932-1936	1938-1944	1932
X Dem	1.40	9.12	-3.79	7.20
V	8.32	3.04	-9.64	2.14
X Rep	-1.56	-9.60	4.01	
V	-16.60	-4.15	13.47	

"As at the national level, electoral change during these years seems to be largely interpretable in terms of southern secession, occupation and Reconstruction" (1980, p. 110).

The late 1890s realignment shows a clear national trend at work. In the 1894 and 1896 elections, the Democrats lost 5.45%, while the Republicans gained 4.39% and the corresponding V s were below ± 3.20 in each case. This contrasts sharply with the pre-realignment period (1884-1892), where the Democrats lost 1.79% swing and the Republicans lost 2.62% swing with V s of -15.41 and -8.21, respectively. The post-realignment period is characterized by smaller swing votes and V s approximately twice as high as during the realignment. The election that shows the most dramatic effect was the 1894 election. This election gave the Republicans a large majority in the House as voters swung away from the Democrats (-7.6%) and to the Republicans (5.5%). The election of 1896, normally considered the realigning election, was at the congressional level an increase for the Democrats as well as the Republicans (3.3% and 3.6% swing, respectively). The fact that both major parties gained in the 55th House elections (1896) over their totals for the 54th House is clearly the result of the Democratic merger with the Populist Party, just as the loss for both parties in the 53rd House reflected Populist party gains in the 1892 election. Thus, the election pair of 1892 and 1894 most clearly meets the criterion for a shift toward national electoral factors over local factors. The most likely explanation for these data lies in an analysis of vote shifts by region, and I shall turn to that analysis in a moment. However, it should be noted that these results support the assertion of Clubb, Flanigan, and Zingale that "the realignment of 1896 appears substantially less impressive than might have been expected" (1980, p. 111).

The results for the New Deal realignment clearly point to the nationalization of electoral factors in the 1932-1936 (73rd through 75th Houses) period. The coefficient of variation is 3.0 for the Democrats and -4.2 for the Republicans. The Democrats gained 9.1% over their pre-realignment vote totals, whereas the Republicans lost 9.6% from theirs. The largest portion of the Democratic gain came in the shift from 1930 to 1932 (7.2%) and the concomitant Republican loss of 8.0%. The mean swings and the V coefficients in the pre- and post-realignment periods support the nationalization hypothesis. In the pre-realignment both parties' mean swing was less than 2.0% and the V s were at least twice as high as during the realignment. In the post-realignment period the mean swings for both parties were approximately 4%, and the V s were between three and four times higher than during the realignment period. These

results for the New Deal are so clear that the only possible explanation is that the New Deal was an across-the-board realignment to Roosevelt and the Democrats. The realignments of the Civil War and 1890s present a more complicated pattern. Thus I turn to an analysis of all three realignments, controlling for regional effects.

A Regional Analysis of Electoral Results

Tables 3-5 show the mean change and variance in pairs of elections for all three realignment eras, controlling for region. If the shift in the vote (swing) was less than 5%, no entry is made in the tables, in order to enhance their readability.³ A coefficient of variation is given for each election pair where the swing was 5 or greater. The expectation is that the Civil War era and the 1890s realignments will be compensating realignments, whereas the New Deal will be an across-the-board realignment. In short, the pattern for the first two realignments will show greater regional variation than will the pattern for the New Deal era.

The results for the Civil War period (Table 3) show both regional variation and compensation. In the New England states, six of the 14 election pairs have mean swings of 5% or greater. Of these, the largest and most significant were the Democrats' loss of 13.5% in the 1853-1854 election owing to the increased vote for the American (Know-Nothing) party and some support for Republican candidates. The election to the 36th House (1857-1858) resulted in another steep drop for the Democrats (-11.3%) as the Republicans became the dominant party. The coefficients for both these elections were below 1.7, which indicates a strong regional over local trend.

The Union Democrats gained 7.8% in the 1862 elections ($V=2.9$), which reflected displeasure with the Union war effort. The Democrats, however, lost most of this gain in the 1864 election, as Union forces were more successful. The only other election pair with greater than 5% swing was the 1872-1874 election pair, in which the Democrats gained 7.2% ($V=1.4$) and brought a Democratic majority to the House for the first time in 14 years.

The pattern for the mid-Atlantic states shows the election for the 35th House (1855-1856) as a realignment away from the Democrats. The Democrats decline from the election of 1853-1854 in the mid-Atlantic states is impressive when one considers that the Know-Nothings made substantial gains in the 1853-1854 elections. In New York,

³The author will provide upon request complete tables showing the results for each election pair.

Table 3. Regional Vote Swings away from Democrats in the Civil War Realignment^a

Congress	Years	New England		Mid-Atlantic		East West Central		South		Border	
		$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y	$\bar{X}\Delta$	Y
31-32	1848-1850	5.5	2.9	13.5	1.1						
32-33	1850-1852							14.3	2.8	6.2	5.8
33-34	1852-1854	-13.5	1.4			-9.7	1.0			11.5	2.3
34-35	1854-1856			-7.6	-2.6			7.9	2.9		
35-36	1856-1858	-11.3	1.6								
36-37	1858-1860										
37-38	1860-1862	7.8	2.9								
39-40	1864-1866										
40-41	1866-1868									9.7	1.5
41-42	1868-1870									8.5	3.7
42-43	1870-1872							14.7	1.9	6.2	3.1
43-44	1872-1874	7.2	1.4					12.6	1.4	7.8	2.7
44-45	1874-1876										

^aOnly average changes of more than 5% are included.

Table 4. Regional Vote Swings to the Republicans in the 1890s Realignment^a

Congress	Years	New England		Northeast		East North Central		West North Central		South		Border	
		$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V
49-50	1884-1886							10.4	2.6	-13.7	1.4		
50-51	1886-1888									11.4	1.6	5.9	2.4
51-52	1888-1890							-8.6	0.7	-9.8	1.9	-6.8	1.9
52-53	1890-1892												
53-54	1892-1894	8.5	0.3	9.1	1.0	6.7	0.8	5.0	0.7			6.1	2.4
54-55	1894-1896	7.1	0.9							10.3	2.1		
55-56	1896-1898	-8.0	0.6	-7.7	2.0					-8.7	-2.2		
56-57	1898-1900												

^aOnly average changes of more than 5% are included.

Table 5. Regional Vote Swings to the Democrats in the New Deal Realignment^a

Congress	Years	New England		Northeast		East North Central		West North Central		South		Border		West	
		$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V	$\bar{X}\Delta$	V
69-70	1924-1926														
70-71	1926-1928											-10.8	1.6		
71-72	1928-1930											11.6	1.5		
72-73	1930-1932			8.6	2.0	12.7	1.3	10.4	1.5					23.6	1.0
73-74	1932-1934														
74-75	1934-1936													6.1	3.3
75-76	1936-1938			-7.9	-0.7	-6.0	1.0							-5.6	2.2
76-77	1938-1940														

^aOnly average changes of more than 5% are included.

New Jersey, and Pennsylvania the Know-Nothings made strong showings in congressional elections on the nativism and anti-Catholic issues. However, by late 1856 the Republicans had become the major anti-Democratic party as the nativism issue faded (Holt, 1978, pp. 160-164).

The election to the 32nd House (1849-1850) resulted in a solid Democratic gain (13.5%) as the Whigs fell apart over the slavery issue and the compromise of 1850. Although no other elections meet the 5% swing criteria, the election to the 39th House (1863-1864) reflected the positive achievements of the Union armed forces, just as the elections of 1861-1862 (38th House) reflected the poor northern showing in the Civil War (Democratic loss of 4.0% and a gain of 3.7%, respectively).

The midwestern states show the election to the 34th House (1853-1854) as realigning. In the elections to the 34th House, the Democrats lost 9.7% from their 33rd House vote totals, and in the elections to the 35th House, the Democrats lost another 4.7%. The pattern reflects first the Republicans replacing the Whigs as the second party in 1853-1854, and then, in the 35th House elections, the Republicans became the dominant party. The Democrats gained votes in the 1861-1862 elections, again as a result of the North's poor showing in the war. When in 1863 and 1864 the North began winning the war, the Republican vote in congressional elections picked up considerably.

The southern states moved solidly into the Democratic column in the elections to the 33rd House (1851-1852) with a gain of 14.3% over their 1849-1850 vote totals. This gain and Democratic dominance in the region were increased in the elections of 1855-1856 (35th House), when the Democrats gained another 7.9% over their 1853-1854 totals. The two lowest coefficients of variation in the pre-Civil War period, 2.8 and 2.9, occur in these two elections. The border states followed the deep southern states into the Democratic camp in the elections to the 34th and 35th Houses. In these two elections, the Democrats gained more than 38% from their 33rd (1851-1852) House totals. The decisive presidential elections of 1860 resulted in another huge gain for the Democrats in the border states. In each of these elections, the V coefficients are below 2.5.

In the post-Civil War period, the readmission of the southern states resulted in an increased Democratic party vote in the period from 1871 to 1874 (43rd and 44th Houses). In these election pairs, 42nd-43rd and 43rd-44th, the Democrats did well. From the 42nd to the 43rd, the Democrats gained in the mid-Atlantic and the southern states—mean change of 8.6% and 10.2%, respectively, and in the 44th House elections, the Demo-

crats gained decisively in the New England (9.8%) and border (7.8%) states.

These results point to a regional and structural explanation of the Civil War realignment. In a varying pattern, regions first abandoned the Whigs for the Republicans, and then the Republicans came, over time, to dominate the northern state elections, while the South turned strongly to the Democrats. The end of the Civil War and the readmission of the southern states brought about solid Democratic gains throughout the country, such that by 1874 (44th House) the Democrats were the majority party in the U.S. House of Representatives. This pattern varied across regions and states as well as across time. Given these regional results, it is obvious why the results for the whole nation do not fit a classic across-the-board realignment pattern.

Regional Results in the 1890s Realignment

The national results showed the 1892-1896 (53rd-55th Houses) election pairs as realigning. The regional results show that the pattern varies by region (Table 4). In the New England states, the elections of 1892-1894, 1894-1896, and 1896-1898 all have low coefficients of variation. The 1894 election has Democrats losing almost 10% from their 1892 vote total, whereas the Republicans gained 8.5% ($V = .3$). In the election to the 55th House, the Democrats lost another 5.5% and the Republicans gained 7.1% ($V = .9$). However, in the election to the 56th House (1898), the Democrats gained back more than 8% of the vote while the Republicans lost 8% from their 1896 totals. The northeastern states follow essentially the same pattern, although swings of more than 5% are not as frequent. In the elections to the 54th House (1894), the Democrats lost 7.6%, while the Republicans gained 9.1%, with V equal to 1.0. Unlike the New England states, northeastern congressional elections did not continue a major swing away from the Democrats in the 1896 elections—a loss of less than 3% for Democratic candidates. In the election to the 56th House, the Democrats gained 7.4% while the Republicans lost 7.7%, with V s of 2.0 or under. Thus, for the eastern states, the critical elections at the congressional level occur in the 1894 to 1898 period, with some differences between the northeastern and New England states. Most of this difference can be accounted for by the resurgence of the Democratic machine in New York City in the 1896 election; that is, while Democrats continued to lose votes in New England in the 1896 elections, the New York City Democrats recaptured votes lost to Republicans in 1894 (Brady, 1973).

Congressional elections in the East North Central states show a separate pattern. In the 1894

elections, the Democrats lost over 10% from their 1892 totals, while the Republicans gained 6.7%. This, of course, follows the national trend. The elections of 1896 show a different pattern in that the Democrats gained votes while the Republicans gained only .3%. The Democratic gain was the result of their adoption of Populist principles and of combining candidates; thus, there was no Republican loss in the vote.

The Populist effect on congressional elections is most evident in the largely agricultural West North Central states. For the Republicans there are three elections where the swing is more than 5% and the V s are under 3.0. Clearly the mean vote swings are high relative to the variance around the swing in these elections. The Republicans gained 10.4% in the 1886 election, lost 8.6% in the 1890 election, and gained 5% in the 1894 election. Most of the vote pattern for this region can be explained in terms of the rise and fall of Populist strength relative to Democratic strength. In terms of a national pattern, the Republican gains in the 1894 House election contributed to the Republicans victory in 1894, but there are no other elections with a mean swing of more than 5%.

The pattern for the southern states parallels that found in the agricultural Midwest. In five of eight cases, the coefficient of variation is below 2.5; in three of these, it is below 2.0. Again, the explanation lies in the rise and fall of Populist strength relative to Democratic strength. Five of the election pairs show swings of more than 8% to or from the Democrats and Republicans. The different pattern observed here for the Republicans, that is, gaining and losing relatively high percentages of the vote, is a result of the fact that the Republicans combined with Populists in a number of southern states to oppose Democrats. After the 1896 election, Democrats met in Jackson, Mississippi, to determine how coalitions of blacks and poor whites could be prohibited from challenging the regular Democratic hegemony in the region. The result was the Jim Crow legislation which caused an immediate sharp drop in voter turnout throughout the deep South and firmly established Democratic control. Evidence for this can be seen in the increase of more than 13% for the Democrats in 1898 and corresponding loss of 8.7% for the Republicans.

The pattern for border-state elections shows relatively small swings in the mean vote except for the 1888-1890 and 1894-1896 election pairs. In the 1888 election, Republicans gained 5.9% ($V=2.9$); however, these gains were lost in the 1890 elections as the Democrats swept to victory. The major change occurred in the elections for the 54th House, when the Republicans gained more than 6% and the Democrats lost more than 9%.

Thus, the border states' electoral voting pattern is more in line with national pattern found in Table 2 than was the case for the southern and West North Central states.

The regional electoral results yield a complicated conclusion at best. The realignment of the 1890s was compensatory in that the north became more Republican while the southern and West North-Central states became more Democratic. However, this pattern varies by region and by election. The New England states swung more solidly Republican than did the northeastern and East North-Central states. The rise and fall of Populist party candidates affected results in the rural East North-Central, West North-Central, and southern states. The brunt of lasting electoral change occurs from 1892 to 1898 with the 54th and 55th House elections benefiting Republicans, and the elections to the 56th House reversing a good portion of the Republican gains. In short, there is no question of a clear across-the-board realignment to the Republicans in the 1890s.

Regional Results in the New Deal Realignment

Unlike the Civil War and 1890s realignments, the New Deal realignment fits the classic pattern. As Table 2 showed, the election pair 1930-1932 was clearly the period where the Democrats became the dominant party. The same analysis run over regions (Table 5) shows some regional variation, but much less than was the case in either of the two previous realignments. The New England states remained Republican even during the New Deal era. The Democrats gained slightly in each election from 1926 through 1932; however, they never gained as much as 3%, and the base from which they started (1924) was very low. Moreover, only one coefficient of variation approaches 2, that for the Republicans in the 1936-1938 election pair. Thus there is little in the data to show a major switch to the Democrats.

The data for the northeastern and East and West North-Central states show a clear switch to the Democrats in the 1932 election. The Democrats gained from 8.6 to 12.7% over their 1930 totals, whereas the Republicans lost between 8.1 and 11.8%. The coefficients of variation in each of these regions for both parties over the 1930-1932 elections are ± 2 or less. No other pre-1932 election pair has a coefficient of variation close to 2. In each of these regions, the Democrats gained votes in each pair of elections from 1924-1926 through 1932. In the Northeast, the Democrats also gained votes in the 1934 and 1936 elections. The result was to transform the northern states from a bastion of Republican strength into a Democratic stronghold from 1932 until 1938.

The congressional elections of 1938 were a modified across-the-board readjustment in the North back toward the Republicans. The coefficients of variation for these states are below ± 2 , and the Republicans gained more than 5% in each region. However, this readjustment did not return the Republicans to their former majority status. Rather, after 1938 the northern states were two-party competitive—a distinct improvement in Democratic fortunes. During the pre-1932 period, the average Democratic vote in these Northern states was 40.3%, and the Republicans averaged 56.8%. The New Deal elections of 1932 through 1938 gave the Democrats an average of 53%, while the Republicans fell to approximately 44%. In the post-1938 electoral period, the parties became very competitive, with the Democrats holding a 48.9 to 47.8% edge through the 1944 elections. In sum, the northern states moved from solidly Republican to two-party competitive as a result of the New Deal realignment.

The southern states began the pre-New Deal period heavily Democratic, and over the New Deal era, there was a slight increase in the Democrats' share of the vote. The only elections with coefficients of variation below 3 are the 1924-1926, 1926-1928, and 1928-1930 pairs. In the 1930 elections, the Democrats gained back the approximately 3.5% they had lost to the Republicans in the disastrous 1928 presidential election. However, owing to Al Smith's candidacy in 1928, Democratic congressional candidates lost almost 4% of their 1926 vote. In 1930, when the party ticket was not led by the antiprohibitionist and Catholic Smith, the Democrats gained 4.4% while the Republicans lost 4.5%. In the 1932 and 1934 elections, Democrats gained votes, but their total percentage was so high that there were no significant coefficients of variation. Thus, the New Deal realignment resulted in a heavily Democratic region becoming slightly more Democratic.

The pattern for the border states shows only two significant pairs of elections, 1926-1928 and 1928-1930, and these results are explainable by the Smith candidacy in 1928. The Democrats lost more than 10% of their vote from 1926 as a result of a backlash against Al Smith. With Smith gone in 1930, the Democrats gained back all they had lost plus approximately one percentage point. In the critical election of 1932, the Democrats gained 3.4%, and the Republicans lost more than 5%. Thus, as in the South, the border states began the period solidly Democratic and ended the period slightly more Democratic.

The western states are more difficult to analyze because the number of representatives in the region is limited and the vote fluctuates wildly, but the 1930-1932 election pair stands out as significant; the Democrats gained more than 23%,

while the Republicans lost over 25%! The Republicans gain back some of this loss in the 1938 elections, but the Democrats end the era in much better position than they began it.

The regional analysis of New Deal elections demonstrates some differences between the extent to which various areas shifted to the Democrats and to a lesser extent the times at which they shifted. However, the major finding was that across all regions there was a shift to Democratic congressional candidates in the 1932 general election, and that in four of the six regions the shift was greater than 8% and the corresponding V s were less than 2.1. The shift was greatest in the non-New England, northern states, and less in the predominantly Democratic southern and border states. Clearly, in comparison to the Civil War and 1890s realignment, the New Deal realignment meets the test of an across-the-board realignment.

Analysis of Variance of Electoral Change

The most thorough test for analyzing realigning elections is the Flanigan and Zingale (1974) technique which sorts out across-the-board and compensating *realignment* change from non-realigning temporary surges and interactive change. Their technique is essentially a two-way analysis of variance (election by subunit) with interaction and one observation per cell (see also Clubb, Flanigan, & Zingale, 1980, pp. 116-117). I duplicated their analysis with the congressional district as the unit rather than the county or the state. This technique allows one to distinguish between temporary and permanent or realigning, across-the-board change and to distinguish between temporary and permanent compensating change. Because I am only interested in realigning change (either across-the-board or compensating), Table 6 shows only results that can be interpreted as realigning. The figures for across-the-board change can be interpreted straightforwardly as percentage-points change. For example, the across-the-board change of 9.6 for the Democrats in 1932 means that the subunits moved an average of 9.6 units toward the Democrats, and the change was not temporary. The interactive-change figures mean that congressional districts moved in different directions from their own expected vote and from the nation as a whole. Because the figures for compensating change represent increases in some districts and decreases in others, no sign is attached to the figure. Following Flanigan and Zingale, I use moving cross sections of four elections.

Table 6 shows that the Civil War era and 1890s realignments are not as clearly realignments as was the New Deal. During the Civil War period, the interpretation is structural. The 3.6 shift away

Table 6. A Flanigan-Zingale Analysis of Variance for Three Realignments

	Democrats		Republicans	
	Across the Board	Compensating	Across the Board	Compensating
Civil War				
1850				
1852				
1854	-3.6			
1856	1.9	2.7		
1858				
1860	-2.8	1.8		
1862				
1864				
1866				
1890s				
1884				
1886				
1888				
1890				
1892				
1894	-3.9		1.9	2.4
1896	1.0			
1898		1.5		
1900				
New Deal				
1924				
1926				
1928				
1930				
1932	9.6	2.1	-4.2	2.3
1934				
1936				
1938	-3.4			
1940				

from the Democrats in the 1853-1854 elections was a result of the Whigs' being replaced in many northern congressional districts. The 1856 elections showed a slight shift (1.9) back to the Democrats and a compensating change of 2.7. The election of 1860, which brought to Republicans control of the government, resulted in a 2.8 loss to the Democrats and a 1.8% compensating shift.

The 1890s realignment showed the Democrats losing 3.9%, while Republicans gained 1.9% across the board in the 1894 elections. In addition, there was a compensating change of 2.4% on the Republican side of the ledger in 1894. The 1896 and 1898 elections showed first a slight Democratic across-the-board shift (1%) and a 1.5% compensating shift in the 1898 elections. In both the Civil War era and the 1890s realignments, there are indications of lasting electoral change;

however, with Clubb, Flanigan, and Zingale (1980), one can conclude: "Electoral change during these years [Civil War era] seems to be interpretable in terms of Southern secession, occupation and Reconstruction. . . . The realignment of 1896 appears substantially less impressive than might have been expected" (pp. 110, 111).

In contrast, the results for the New Deal realignment indicate a clear-cut across-the-board shift toward the Democratic party in the 1932 election. The across-the-board 9.6% Democratic gain and the 6.2% Republican loss clearly outweigh the 2.1 and 2.3% compensating losses. The Republican resurgence in the 1938 elections is documented by the across-the-board, 3.4% loss suffered by the Democrats in the 1938 elections.

The major point of this extended analysis of the three major American realignments is that at least two of the three realignments were not the result

of massive shifts of voters toward the new majority party. I do not deny that each of these were realignments; rather the point is to show that what is important about realignments is how the electoral results affect the composition of the U.S. House of Representatives. The most important effect is that in each realignment the result was undisputed control of the House of Representatives, the Senate, and the presidency for at least 14 years. The massive shift of voters to the Democratic Party in the 1932 elections is easily translated into Democratic control of the House. The Democrats added to their solid southern and border-state base a bloc of urban, northern ethnic districts (Brady & Stewart, 1982). It is more difficult to understand how the less dramatic electoral shifts in the Civil War era and the 1890s resulted in Republican control for an equal period. The answer to this question entails analysis of the votes-to-seats ratio and the use of the so-called cube law (Tufte, 1973).

The Votes-to-Seats Ratio

In the American electoral system, representatives are elected through a single-member, pluralist winner arrangement. This "first past the post" rule often distorts the ratio of popular votes to legislative seats. In attempting to assess this distortion, scholars in the United States and Great Britain (which have the same electoral arrangement) have tried to apply the cube law, which states that the votes-to-seats ratio will take the following form:

$$\frac{\text{seats Party A}}{\text{seats Party B}} = \frac{\text{Votes Party A}^3}{\text{Votes Party B}^3}.$$

Although the cube law has not attained the status of a precise empirical law, some, for example, argue it should be a $2\frac{1}{2}$ rule. It does contain much validity in that for some periods, it expresses the votes-to-seats ratio accurately. The argument in this section is that during the major electoral realignments in the United States, there is a high votes-to-seats distortion. I shall use the cube law, which represents a significant distortion, as a base line. The hypothesis is that during realignments, the votes-to-seats ratio will be greater than that predicted by the cube law. This is particularly important for the Civil War and 1890s realignments, because as I have shown, there is not major shifting of votes to Republicans during either of these realignments. In short, the long periods of Republican control brought about during the Civil War and 1890s realignments were more the results of a vote-to-seat distortion than of a massive voter shift, as was the case in the New Deal realignment.

The regionalism of both the Civil War and 1890s realignments was the result of an overall shift of approximately 5% of the voters to the Republicans in the northern states. This shift causes the competitive seats to become Republican and translates this vote shift into sizable seat gains for the Republicans. In the New Deal realignment, the vote shift in the north to the Democrats should result in a significant vote-seat distortion which should be masked when the solidly Democratic South is calculated into the equation. In the Civil War realignment, the period immediately before the realignment (the Second Party system) was characterized by a nationally competitive two-party system, and from 1860 to 1868-1870, only the northern states are in the Union; thus, during this period, calculations are not divided regionally. In the 1890s and New Deal realignment, the calculations are done both with and without the South. Table 7 shows the results for all three realignments.

The results for the Civil War realignment corroborate the hypothesis. In the elections to the 34th House where the Republicans become the plurality in the House, they enjoyed a +.50 differential above what the cube law predicted. The 1855-1856 elections were accurately predicted (-.05), whereas in the 1857-1858 elections, the Republicans were again benefited (.53). The major distortion in the series occurs in the presidential election year of 1860. The cube law predicts a vote-to-seat ratio of 1.07 for the Republicans, and the actual seat ratio was 2.52, which favors the Republicans by an impressive 1.45 over the cube law prediction. Thus, the Republican victory in 1860 grossly distorted the seats in the House toward the new majority party. After the southern secession, the cube law becomes a more accurate predictor, with no post-secession House having an advantage as great as .50. The swing in votes to Republicans in the North during the 1859-1860 elections clearly results in an overwhelming number of competitive seats going Republican. In the elections that follow, the Republicans retain their majority, but by narrower margins. In fact, during the election to the 38th House (1861-1862), the Union Democrats almost regain control of the House of Representatives.

The national elections of 1886 and 1888 (50th and 51st Houses) favored the Republicans by a .46 and .36 margin over the cube law prediction. The largest difference between the cube law prediction and actual results appears in the 1894 elections (54th House), when the Republicans have a full 1.0 difference between the predicted seat ratio and the actual results. The elections to the 55th, 56th, and 57th Houses (1896 to 1900) also gave the Republicans a sizable advantage (.46, .46, and

Table 7. The Votes-to-Seats Ratio in the House of Representatives during Three Realignment Eras^a

Realignment Year	Cube Law Prediction		Actual Results		Difference	
	With South	Without South	With South	Without South	With South	Without South
Civil War (R to D)						
1854	.80		1.30		+ .50	
1856	.77		.72		- .05	
1858	.59		1.12		+ .53	
1860	1.07		2.52		+1.45	
1862		1.18		1.29		+ .11
1864		3.21		3.15		- .06
1866		2.09		2.92		+ .37
1890s (R to D)						
1884	.54	.89	.76	1.20	+ .22	+ .31
1886	.42	1.05	.88	1.38	+ .46	+ .33
1888	.66	1.22	1.02	1.73	+ .36	+ .51
1890	.35	.76	.36	.51	+ .01	- .25
1892	.42	.89	.55	.83	+ .13	- .06
1894	1.05	2.66	2.05	5.70	+1.00	+3.04
1896	1.05	2.20	1.51	2.93	+ .46	+1.73
1898	.58	1.52	1.04	1.83	+ .46	+ .31
1900	.76	1.95	1.19	2.36	+ .43	+ .41
New Deal (D to R)						
1924	1.18	.33	.76	.38	- .42	+ .05
1926	1.27	.34	.81	.42	- .46	+ .08
1928	1.08	.31	.61	.28	- .47	- .03
1930	1.68	.48	.99	.56	- .69	+ .08
1932	4.78	1.58	2.38	1.63	-2.40	+ .05
1934	5.41	1.84	2.88	2.07	-2.53	+ .23
1936	5.60	2.15	3.38	2.47	-2.23	+ .32
1938	3.52	1.08	1.49	.96	-2.03	- .12
1940	3.35	1.08	1.63	1.05	-1.72	- .03

^aTheory dictates use of all elections before 1862.

.43). Excluding the southern states from the analysis clearly shows that the pro-Republican seat distortion comes from the northern states. In the 1894 elections in northern states, the cube law predicts a Republican seat advantage of 2.66 to 1, whereas the actual seat ratio is 5.7 to 1, a difference of 3.04 favoring the Republicans. The election of 1896 in the northern states also heavily favored the Republicans, a 1.73 advantage over the cube prediction. Thus, in the two critical elections of 1894 and 1896, the Republican victories in the House can be attributed to the vote-to-seat advantage they enjoyed in the northern states. It is worthwhile to point out that the cube law predictions are in fact sizable distortions themselves; thus, when a party has advantages over the prediction, the electoral system is creating a majority party. In sum, the realignment to the Republicans in 1894-1896 can best be understood not in terms of massive vote shifts, but rather in terms of relatively minor across-the-board shifts to the

Republicans in the northern states which translated into major seat shifts.

The results for the New Deal realignment show the Republicans with a clear national advantage throughout the period. In every election from 1924 to 1940, the Democrats received fewer seats than the cube law predicted. From 1924 to 1930, when the Democrats were generally not competitive in the North, the Democrats were from -.42 to -.69 from their predicted number of seats. Beginning with the 1932 realignment, when the Democrats clearly became the majority party, they were considerably below the cube law prediction. In fact, from 1932 until 1940, the Democrats were from 2.53 to 1.72 below the cube prediction. This is understandable in that the Democrats control the South so heavily that the addition of northern voters leaves them below what the cube law predicts.

Excluding the South demonstrates these points clearly. In the northern states, the cube law is ac-

tually a good predictor of the vote-to-seat ratio. In the 1932 elections, the Democrats are .05 over the cube law prediction; however, in both the 1934 and 1936 elections, the Democrats receive a .25 and .32 average over the cube law prediction. In short, the across-the-board 1932 realignment to the Democrats was so massive in proportion that the Democrats dominated without a votes-to-seat distortion equal to the cube prediction. Since the Democrats gained in every region, the cube law actually overpredicts the actual vote-to-seat ratio.

These results point to differences between the nineteenth-century realignments and the New Deal realignment. This difference has been noted by other scholars (Burnham, 1970, 1974; Converse, 1974; Rusk, 1970, 1974). Burnham's (1970) thesis attempts to account for these differences by claiming that the nineteenth century was a different political universe in which voter interest and participation were high. Moreover, the party systems were characterized by strong, highly divided partisan blocs with relatively little give in the electorate and no large bloc of uncommitted voters. The break-up of the nineteenth-century system resulted in a steady decline in voter participation, partisanship, and ultimately policy consensus in government (Burnham, 1965, 1970). Thus, the New Deal realignment was just a temporary break in the trends of declining partisanship and participation. The argument offered in this article is simpler and does not carry with it any ultimately untestable baggage such as the differing effects of education on politics in the nineteenth and twentieth centuries. Rather, the argument offered herein has been that in a competitive party system, relatively minor shifts of votes in the 1850s and 1890s resulted in clear majorities for the new majority party. This argument has the advantage of cross-time and cross-national comparability; that is, it is an argument about certain measurable kinds of party systems yielding certain results. Thus, the difference between nineteenth- and twentieth-century realignments are accounted for by the following two factors. First, the nineteenth-century realignments were compensating in that the Republicans gained in the North and lost in the southern and border states. Compensating realignments would be less likely to show as dramatic electoral effects as would across-the-board realignments. Second, in both the Civil War and 1890s realignments, the average margin of victory before the realignment was relatively small; thus minor swings in the vote could result in major seat swings in the seat ratios. Although one cannot conclude from this much about the changing effect of education on participation or on the role of politics as entertainment, one can draw generalizations about the effects of small swings of the vote in a competitive party system.

Namely, competitive party systems can have significant seat swings given only minor vote switches if the vote swing cuts across a large number of districts.

A partial proof for this generalization can be derived by looking at the ratio of vote shifts to seat shifts in British general elections; that is, the British party system before 1970 was similar to the nineteenth-century American system in that there were strong partisan blocs, little relative give in the electorate, and no large bloc of uncommitted voters, plus both systems have a single-member, plurality-winner electoral arrangement. Thus we would expect that under these conditions small shifts in votes would often result in major seat switches. In 8 of 18 elections from 1900 to 1970, vote swings of less than 7% resulted in seat swings of more than 20%. Moreover, the elections of 1910, 1924, 1929, and 1970 had vote swings of less than 5% with seat swings ranging from 15 to 32%. (See Brady & Stephens, 1983, for a more complete analysis.) In short, the party system factor is related to the electoral results. The Civil War and 1890s realignments occurred during a period in which there existed relatively strong party systems; thus small shifts to the Republicans in the North resulted in seat changes in the House which kept Republicans in control for over a decade. The New Deal realignment was a major electoral shift to the Democrats in the North which, when added to their solid southern and border-state base, gave Democrats control of the House for over a decade. The important point is that however the House majority was created, by major vote switches or a vote-seat distortion, the result was that rare condition in American politics, unified control of government for over a decade.

Party Structure of Voting across Realignments

The electoral effects in all three realignments resulted in unified majority parties capable of enacting the clusters of policy changes associated with these realignments (Brady, 1978, 1982; Clausen, 1975; Ginsberg, 1972, 1976; Sinclair, 1977, 1978). One of the major reasons for the high levels of party voting associated with the clusters of policy changes is that the "new" congressional majority was elected on relatively "national" issues. Even in the compensating elections of the Civil War and 1890s era, the Republican congressional party was elected to majority by standard swings in the northern states. Under such conditions, constituent-party cross pressuring is reduced because the election was decided on the national issues of the realignment (Brady, 1978, 1982; Huitt, 1961). This argument has been elab-

orated in detail elsewhere; thus I simply present here three tables showing the product moment correlation between party identification and the issue dimensions associated with each of the three realignments. Details regarding the construction and measurement of the issue dimensions can be found in Brady (1982). Tables 8 through 10 show the results.

The findings show that during each realignment, party structures voting patterns, especially in regard to the dominant issues of the realignment. This result was expected, given the national nature of the realigning elections; however, an important aspect of how realigning elections are converted into policy changes has been neglected. Namely, how were committees affected by three elections such that they legislate clusters of policy changes?

Committees and Policy Changes^a

It has been shown that during realigning eras, turnover on congressional committees was extremely high (Brady, 1978). When turnover on committees is high, committee stability and its ability to pass along the norms that affect decisions is sharply altered. Fenno (1966), for example, has shown that high turnover on the Appropriations Committee affected committee behavior. Specifically, he argued that when the Republicans became the majority in the House

^aI am indebted to Joseph Cooper for help with this section. He and I have written a forthcoming article that covers the appropriations process more thoroughly, including an Arima analysis.

Table 8. Product Moment Correlations between Party Identification and Issue Voting over the 33rd (1853) to the 42nd (1871) Congresses

	33rd	34th	35th ^a	36th	37th	38th	39th	40th	41st	42nd
Slavery secession and civil rights	.51	.41	.89	.87	.88	.92	.91	.96	.98	.97
Public works	.65	.59	.89	.78	—	.67	.78	.64	.91	.91
Railroad and telegraph construction	.14	.22	.89	.67	.32	.58	.52	.15	.38	.47
Housekeeping	.27	.47	.96	.90	.64	.92	.79	.94	.97	.94
Money and banking	—	—	—	.86	.79	.94	.41	.78	.83	.94
Tariff	.55	.02	.71	.85	.74	.92	.71	.58	.79	.59

^aA rise in correlation of party identification with issue dimensions is expected at this point in time.

Table 9. Product Moment Correlations between Party Identification and Issue Voting from the 52nd (1890) to 56th (1900) Congresses

Issue Dimensions	Congress				
	52nd	53rd	54th	55th	56th
Currency	.02	.42	.71	.96	.96
Military Appropriations	.69	.19	.16	.89	.93
Tariff	.97	.89	.20	.98	—
Business	.92	.78	.41	.92	.98
Public Works	.31	.70	.26	.31	.31
Housekeeping	.73	.93	.94	.88	.99
Immigration	.26	—	.47	.54	.81
Taxation	—	.97	—	.92	.88

Table 10. Product Moment Correlations between Party Identification and Issue Voting from the 69th (1924) to 76th (1940) Congresses

Issue Dimensions	Congress							
	69	70	71	72	73	74	75	76
Government management	.84	.88	.91	.94	.97	.91	.94	.89
Social welfare	—	—	—	.72	.89	.94	.86	.69
Agriculture assistance	.08	.24	.94	.77	.90	.91	.77	.89
Civil rights	.17	—	.20	.46	—	.22	.18	.22

after the 1946 elections, they attempted to cut back many of the New Deal programs. In the process, consensual norms that had governed committee behavior were threatened. Thus, there is some empirical evidence to justify using turnover as a variable that affects policy outputs.

A second variable that affects committee behavior is partisanship. The Appropriations Committee integrates partisan and ideological differences by attempting to build a consensus (Fenno, 1973). The committee is successful—wins the floor—when the appropriations bill is viewed as a committee bill rather than a Democratic or Republican bill (Fenno, 1973). In contrast, the Education and Labor Committee, among others, has a very partisan decision style (Fenno, 1973; Murphy, 1974). The result is that the Education and Labor Committee reports partisan and ideological bills which are not as easily passed on the floor of the House. In this section I examine the effects of realigning elections on the House Appropriations Committee.

The Appropriations Committee is at the heart of the congressional process and is thus a natural choice. In the post-World War II era, the Appropriations Committee has operated in a consensual mode wherein differences are compromised within the committee. This process of consensus building results in incremental public policy changes (Fenno, 1966, 1973). Moreover, after the formation of the Appropriations Committee in 1865, the major changes in the House's appropriations process resulted from disputes over the committee's funding of programs and agencies. When the House in the 1880s gave seven committees besides Appropriations the right to appropriate funds, it was because the Appropriations Committee was not funding agencies such as agriculture at a high enough level. When the House voted in 1919 to return all appropriations powers to the Appropriations Committee, it was because the seven clientelist committees were funding their agencies and programs at too high a level. Thus there exists historical evidence that the committee's strategy to report consensual bills that balance the need to guard the Treasury and to fund programs incrementally is a long-standing strategy.

During realigning eras, old policy patterns are broken and clusters of policy changes are enacted. If this is the case for policy, in general such results should also be observable at the level of House appropriations. The argument is that if the decision process on the committee is partisan rather than consensual, the chances of nonincremental appropriations is increased. In addition, I argue that during realignments, committee turnover is high and the new members of the Appropriations Committee will behave in a partisan fashion, and

the result will be appropriations which are non-incremental. In order to demonstrate this, it is necessary to show that:

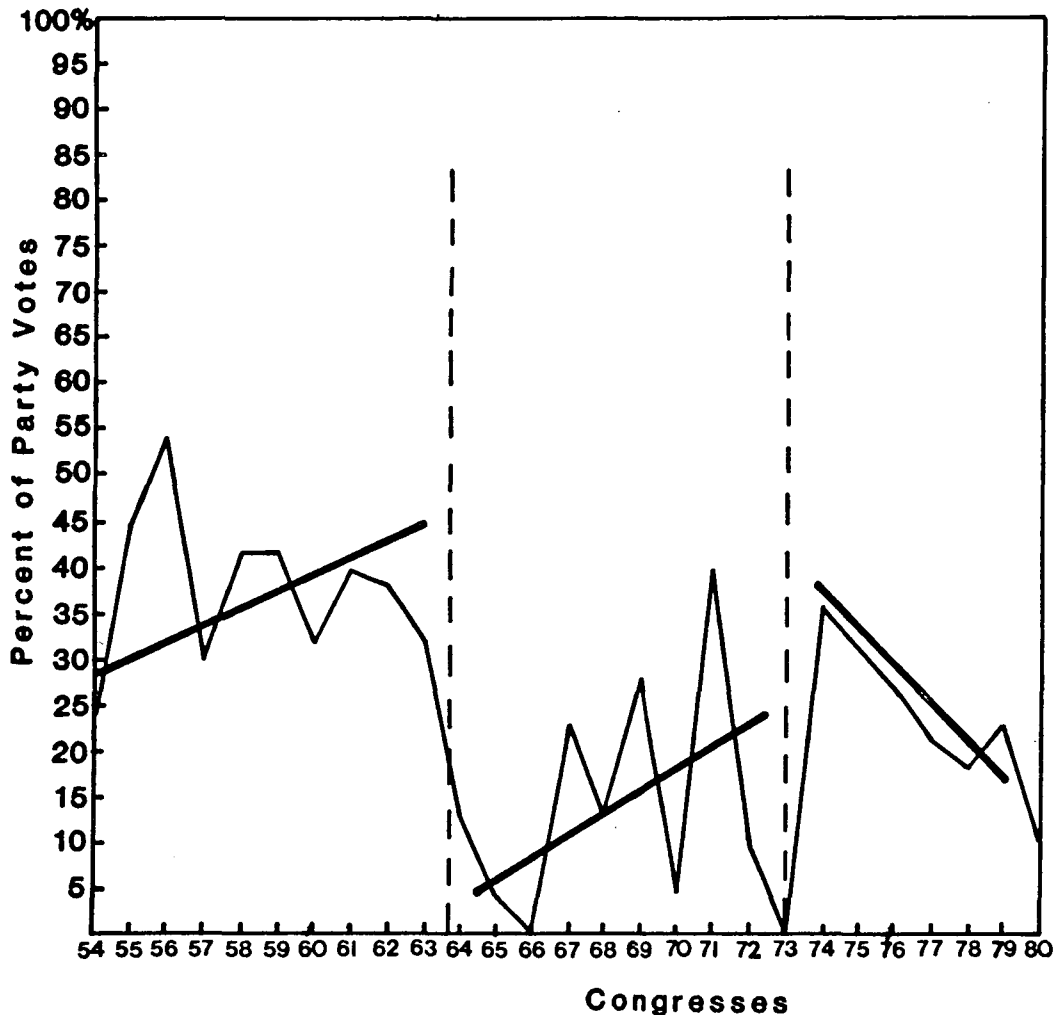
- 1) During the period from 1890 to 1950, the Appropriations Committee's behavior was at times partisan as well as consensual.
- 2) The absolute change in appropriations from year to year is associated with levels of partisan voting. Specifically, partisan committee behavior will be related to nonincremental appropriations.
- 3) During the realignment periods 1895-1899 and 1933-1939, the levels of partisanship and non-incrementalism will be highest.
- 4) Committee turnover will be high during the realignment periods and thus associated with both partisanship and nonincrementalism.

In sum the argument is that during a realignment period, electoral results cause high turnover, and the new Appropriations Committee behaves in a partisan fashion which results in nonincremental appropriations.

The 1890-1910 era in congressional history was the heyday of the Speaker's power. The committee and party systems were molded together by the practice that party leaders chaired the important committees. The result was a more hierarchical, centralized House of Representatives (Brady, 1973; Brady & Althoff, 1974; Cooper & Brady, 1980; Jones, Cannon, & Smith, 1968; Polsby, 1960; Polsby, Gallaher, & Rundquist, 1969). During this era, party voting in the House of Representatives peaked; thus one expects to find that during this period the House Appropriations Committee voted in a more partisan fashion. In order to test this hypothesis, Appropriations Committee members' votes on the following kinds of roll calls were analyzed: amendments to appropriations bills, recommittal motions on appropriations bills, and the single vote on final passage. An average Index of Party Likeness score was computed for the entire committee, and the percentage of all such votes where 90% of one party opposed 90% of the other party was calculated. These scores were calculated for all Houses from the 54th through the 81st House (1894-1950). Figure 1 shows the percentage of 90 versus 90 votes on the Committee over time.

Figure 1 clearly shows that over the entire time series, there have been periods of partisan committee-member behavior. The period from the 1894 realignment to the stripping of the Speaker's power in the 61st House is a period of consistently partisan behavior. The lowest figure occurs in the 54th House (22%), whereas in the 55th and 56th Houses, the average is 50% of all votes being heavily partisan. During the second period, the

Figure 1. Percentage of Party Votes (90 vs. 90%) on House Appropriations Committee, 1894-1950



average is much lower, and only two Houses attain levels corresponding to the 1894-1911 levels. The New Deal realignment results in an increased percentage of party votes during the 1934 to 1938 period (74th through 76th Houses), when Roosevelt made his turn to the left (Leuchtenberg, 1963). After these Houses, the figures fall back toward the levels Fenno (1966) reports. In addition, I calculated an average Index of Party Likeness for each House, which was 27.2 for the 1894-1915 period and 51.6 for the post-1916 period. Thus, by both measures it is clear that the Appropriations Committee members have behaved at various points in a partisan as well as a consensual manner.

The partisan behavior of the committee occurred during two periods: the 1894-1911 period, when party strength was greatest in the entire House as well, and during the New Deal realignment era. The 1894-1915 period contains both a realignment and a post-realignment period when committee partisanship was high, which suggests two important points about partisanship and non-incremental appropriations decisions. I expect that during the realignment period 1895-1899 changes in appropriations would be greater than they were in the post-1900 period, even though partisanship remains high. Essentially this means that partisan behavior can yield both nonincremental and incremental decisions. The nonincre-

mental decision will occur in realignment periods when committee turnover is greatest. If I can show that the 1895-1899 and 1933-1939 periods were the most partisan and also the least incremental and that turnover in these periods was greatest, the results for the committee study would be similar to those found for the entire House.

Estimating the amounts of money appropriated is problematic during the First and Second World Wars, and to a lesser extent during the Spanish-American War. In order to reduce as much as possible the effects of war on appropriations decisions, I do not include defense appropriations in any of the 58 appropriations bills in the time

series. In addition, I calculated changes in appropriations for agriculture, rivers and harbors, commerce, and labor as a check against changes in total appropriations. Because these checks did not show significant differences from the overall appropriations, only the overall figures (minus defense) are given. Figure 2 shows the absolute changes from appropriations by previous Houses for the entire time series excluding World Wars I and II. The figure for each Congress is the average of the two appropriations bills passed by the House.

The results show that the highest absolute changes in appropriations occur during the realignments of the 1890s and 1930s. The only

Figure 2. Percentage Changes in Appropriations Committee Decisions, 1894-1950

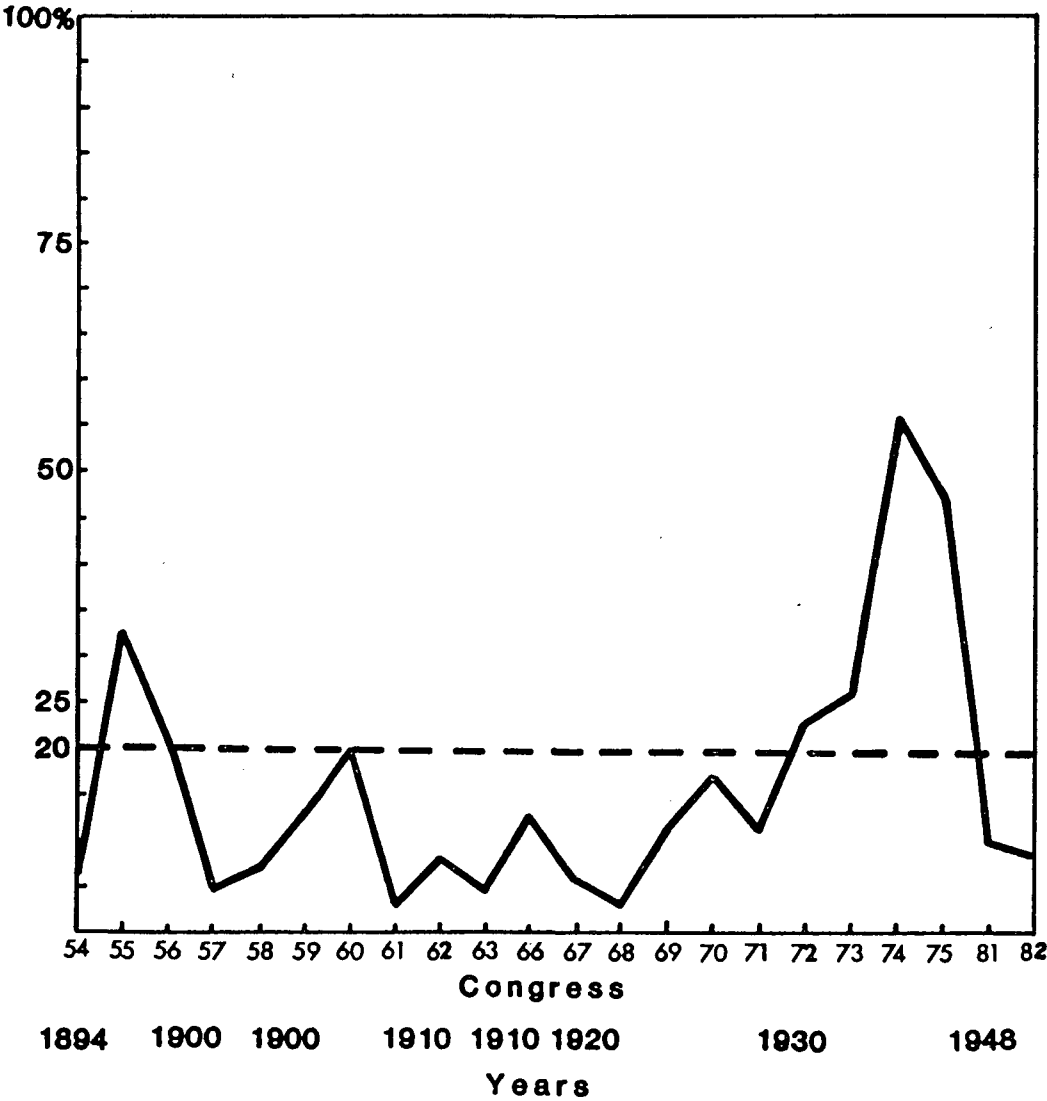


Table 11. Appropriations Committee Turnover, Party Voting and Changes in Appropriations, 1895-1949

	Committee Turnover		Average Index of Likeness		Average % Increment	
	$\bar{x}$	σ	$\bar{x}$	σ	$\bar{x}$	σ
1895-1899	46.6	11.5	20.5	0.7	27.0	8.5
1900-1915	35.9	18.1	34.3	4.2	7.6	7.1
1916-1932	26.0	13.6	57.6	12.4	4.5	18.9
1933-1939	34.3	10.9	29.7	1.2	34.1	19.4
1940-1949	24.4	8.0	54.0	9.3	9.6	14.3

changes in excess of 20% obtain in the realignment eras. The single exception is the change of over 20% in the 72nd House (1930), and that change was a decrease of more than 20% in President Hoover's attempt to balance the budget. The only positive changes above 20% are associated with the two major post-Civil War realignments.

At this point, I have shown that the Appropriations Committee has behaved in a partisan fashion during both realignments and during the period from 1900 to 1915 and that absolute changes in appropriations are greatest during realignments. What remains is to tie together committee turnover, partisanship, and major changes in appropriations. Table 11 shows the average turnover on the Appropriations Committee, the average Index of Party Likeness for committee members on relevant votes, and the average positive increases in appropriations. The hypothesis is that during the 1895-1899 and 1933-1939 realignment periods committee turnover, party voting, and appropriations will be high, and that high partisanship during the 1900-1915 period will not result in abnormally high change in appropriations. In addition it is expected that the 1916-to-1932 and 1940-to-1949 (excluding war expenditures) periods will follow the Fenno model of lower partisanship and incremental changes in appropriations.

The results support the argument. During the 1890s realignment partisanship was high with an average Index of Likeness of 20.5, which means that the average vote was about 80% of Republicans opposing about 80% of Democrats. The average increase in expenditures during these years was 27.0%, and the turnover on the Appropriations Committee averaged 46.6%. The New Deal realignment was also characterized by high turnover—34.3% party voting—Index of Likeness of 29.7 (about 75% of one party versus 75% of the other), and an average increase in expenditures of 34.1%. Thus the highest partisanship and highest change in appropriations occurred during realignment periods. The turnover on the Appropriations Committee during the 1900-1915 period was slightly higher than that found during the

New Deal realignment. However, it must be remembered that during more than two-thirds of this period the Speaker had the power to appoint committees. The 1895-1899 and 1933-1939 periods were highest in terms of electoral turnover; that is, incumbent members defeated in an election. Thus the slight difference observed in turnover between the 1900-1915 and 1933-1939 periods can readily be accounted for by internal structural factors. Turnover on the committee brought about by electoral results appears to bring about the most dramatic changes in partisanship and changes in expenditures.

The 1900-1915 period is characterized by relatively high partisan voting, but the average increase in appropriations is only 7.6%. The 1916-to-1932 and 1940-to-1949 periods have the lowest turnover, the lowest levels of partisan voting, and average increases in appropriations below 10%, a figure commonly used to define incrementalism.

Conclusions

There are two sets of findings in this article. The first is that there are similar linkages present in all three major American realignments. Realigning elections at the congressional level are determined more by national issues than by local factors. The electoral turnover generated by the realigning election(s) results in a congressional majority party that behaves in a partisan fashion to achieve major policy changes. The article shows that whether the unit of analysis is congressional voting on roll calls or Appropriations Committee money decisions, the electoral turnover generated by the realignment results in an American form of party government. The reduction of constituent-party cross pressuring and the sense of mandate from the election results creates a new majority party capable of legislating major shifts in American public policy.

The second set of findings shows that at least two of the major American realignments do not meet a number of the criteria normally associated with electoral realignments. Neither the Civil War nor 1890s realignments were of the magnitude or

nature of the New Deal realignment. In both these realignments relatively minor shifts of voters to the Republicans in the North resulted in long periods of Republican dominance of government. In large part, both the original electoral success and subsequent control were the result of a vote-seat distribution that favored Republicans. These vote-seat distortions especially favored the Republicans in the realigning election or elections. Thus, although there appears to be structural similarities across all three realignments, it should also be clear that interpretations of American realignments that automatically ascribe policy results to massive voter shifts are facile.

Like almost all research on realignments, this article has focused on electoral and structural change. Very little attention has been paid to the role of congressional leadership in obtaining these policy shifts. There have been other periods in American political history where the electoral and structural shifts associated with policy change have been present (Clubb, Flanigan, & Zingale, 1980). The Republicans' victory in 1920 and their domination of government for a decade comes to mind, yet we know that there are no major shifts in policy associated with this period. Why some periods result in major policy changes while others do not remains an unanswered, even an unasked question. Certainly the way congressional leaders interpret election results and how they translate their perceptions into policy greatly affects policy outputs. Future research will have to focus on the role of congressional leadership in formulating and adopting policy responses to critical elections. Franklin Roosevelt is unanimously viewed as the architect of the New Deal; yet we know that Congress forced some policies, the Wagner Act for example, on a reluctant Roosevelt. Until we know how Congress itself affected the policy process during such eras, we will continue to have a view of realignments that overemphasizes the effects of both elections and presidents.

Reducing the explanatory strength of electoral and presidential variables allows us to concentrate on how electoral results change congressional structure and agendas. Focusing on structural and agenda variables begins to tie realignment studies to the social choice results generated by Shepsle (1979) and Ferejohn (1974).

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Perceived Representativeness and Voting: An Assessment of the Impact of "Choices" vs. "Echoes"

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The persistent decline in voting in presidential elections since 1960 has resulted in serious scholarly attention being given to nonvoting. Despite the quality of these studies, however, the ratio of what we know about nonvoting to what we do not know is rather low. In the hopes of improving this situation, I advance the hypothesis that one reason that individuals do not vote is that their interests are not represented by any of the major candidates. To test this hypothesis, I used the SRC election studies (1968-1980) to construct measures of individuals' perceived distance from the major candidates on a variety of issues. Net of an extensive set of factors usually invoked to explain participation, increased distance from candidates significantly decreases the probability of voting. The implications of these results are discussed.

The persistent decline in voter turnout in presidential elections since 1960 has resulted in serious scholarly attention being given to nonvoting. There is now a growing number of studies that attempt primarily to explain this trend and secondarily to explain nonvoting in general (e.g., Abramson & Aldrich, 1982; Boyd, 1981; Cassel & Hill, 1981; Cavanagh, 1981; Hadley, 1978; Katosh & Traugott, 1982; Ladd, 1978; Reiter, 1979; Rosenstone & Wolfinger, 1978; Schaffer, 1981; Tarrance, 1978; Wolfinger & Rosenstone, 1980). Despite the quality of these studies, serious gaps in our knowledge of nonvoting still exist, and I hope that this study will begin to fill some of them.

Recent nonvoting studies have emphasized three principal kinds of factors: 1) socioeconomic and demographic (e.g., income, education, age, and sex); 2) social psychological (e.g., party identification, citizen duty); and 3) rational choice variables (e.g., the perceived closeness of the election) (Cassel & Hill, 1981; Cavanagh, 1981; Reiter, 1979; Shaffer, 1981). The procedure for testing these relationships is generally the same: through some extension of the general linear model, a dichotomous dependent variable of voting-nonvoting is related to some subset of known or suspected correlates. These studies suggest that the

most important factors contributing to the decline in turnout have been the changing age structure (see also, Boyd, 1981), a decrease in participation among lower status citizens with no counterbalancing increase, and declines in both political partisanship and feelings of political efficacy.

Despite their value, however, these analyses have a major shortcoming. In line with the dominant emphasis in electoral research in the United States, they focus primarily on individual factors and do not consider the importance of the options available to individuals. Turnout among any segment of the electorate is known to be decreased by the absence of a realistic party or candidate choice expressing the interests of that segment (see Milbrath & Goel, 1977; Zipp & Smith, 1982), but this finding has not been incorporated into studies of recent voting declines. In this article, I present evidence of how consideration of the options available to individuals helps to explain nonvoting.

Background

Converse (1971) listed three broad factors that keep people from voting: accidental, legal, and motivational. Accidental factors do not concern us because there is little that can be done about them. Legal factors, on the other hand, clearly explain some portion of nonvoting, as it is harder both to register and to vote in the United States than in virtually any western democracy. However, legal factors cannot explain the decrease in turnout since 1960 because since that time legal restrictions on registration and voting have been relaxed (e.g., Rosenstone & Wolfinger, 1978). The third set of factors, motivational ones, is generally considered to be the most important. Converse surmised that typically motivational factors in-

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volve some indifference or apathy to voting. He further distinguished two types of motivational factors: internalized motivation (the learned need for citizen participation), and external stimulation (the mobilizing effects of the excitement surrounding a presidential campaign).

Although attention has been given to both of these aspects of motivation, perhaps much more has been given to internalized motivation. Brody (1978), Cassel and Hill (1981), Cavanagh (1981), Reiter (1979), and Schaffer (1981) all cite the expectation that increased levels of education during this period should have resulted in increased turnout. In other words, because more people became aware of the need for and the benefits of participation, more people should have voted. The effects of education, however, may have been offset by external factors: the enfranchisement of 18-year-olds and the increased distrust of and cynicism toward the government in the wake of the Vietnam war and Watergate. Lowering the voting age increased the size of the electorate and artificially lowered turnout rates (when compared to earlier years), because 18-20 year olds have low rates of turnout (e.g., Boyd, 1981). The same cannot be said for political trust, as Schaffer (1981) found no significant independent effect of trust on turnout.

There is one major external factor which generally has been ignored. After finding that lower status whites had higher rates of nonvoting than others, Reiter (1979) drew on Burnham (1967) and mentioned that a reason for this failure to vote might be the lack of a viable socialist party for them to support. In comparative terms, the socioeconomic gap in participation is greater in the United States than in most other western democracies (Verba, Nie, & Kim, 1978; Zipp, Landerman, & Luebke, 1982). Although this is a provocative idea, there has been no direct test of it in the United States and, indeed, Reiter (1979) went so far as to speculate that such a test might not be possible with current data.

Although it is not my purpose to determine whether or not lower status citizens want a socialist party, I do contend that turnout among various groups in the electorate is affected by the presence or absence of a party that represents the interests of those groups. In their study of voting behavior in Canada, Zipp and Smith (1982) found that nonvoting among members of the working class decreased in constituencies in which there was a viable candidate of a mildly socialist party (the New Democratic Party), the party that they presumed to represent the interests of the working class. Furthermore, research done by Campbell and his colleagues, along with others, has found that turnout increased when people perceived clear and attractive alternatives in a particular

election (Campbell, 1960, 1962; Campbell et al., 1960; Rokken & Valen, 1962). Based on this work, I have tested the hypothesis that accounting for whether or not one's interests are represented among the various candidates increases our ability to explain turnout.

Data and Methods

The data for this article are drawn from the Survey Research Center's American National Election Studies. These data are available from the Survey Research Center at the University of Michigan and are a standard source for studies of voting in presidential elections.

Because my hypothesis concerns the idea that the options available to individuals affect voting, a central task is to obtain measures that indicate whether or not the respondents felt that their interests were represented by any candidate in the relevant election. Although there is no direct information on this, I was able to construct measures that reflect on a 7-point scale the distance between the respondent's position on a particular issue and the respondent's perception of each candidate's position on that same issue. The closer a respondent is to a candidate, the more that respondent feels his or her interests on that issue are represented by that candidate. Perhaps the situation most conducive to high levels of voting would occur when all citizens are close to one candidate but a considerable distance from the others, so that there would be clear, representative choices for everyone. The least conducive situation, on the other hand, would occur when all individuals are equidistant from all candidates. In the latter case, individuals would be indifferent about who wins the election, whereas in the former instance, who wins would make a difference to the voters. What I have outlined, in fact, is very similar to Brody's and Page's (1973) notion of indifference. Drawing on the idea of rational abstention (Downs, 1957), Brody and Page demonstrated that those who felt equally about the candidates were less likely to vote than those who clearly favored one candidate.¹ Thus, *indifference* is one way in which the lack of representation can lead to nonvoting.

As Brody and Page also noted, it is possible to have a relatively clear choice but yet, at the same time, not to feel favorably disposed toward any of the candidates. Brody and Page termed this notion, which draws most closely from Con-

¹Brody and Page (1973) did not use these perceived distance measures, but rather used a summary evaluation of how favorably the respondent felt toward each candidate.

verse's (1966, p. 24) idea of "dynamic nonvoting," *alienation*. Brody and Page found that even though alienated citizens might not be indifferent, they were also less given to voting. Thus, it is through both indifference and alienation that the absence of preferred options can lead to nonvoting.²

The questions on which the measures of both indifference and alienation are based are listed in the Appendix. Because these questions were included only in the SRC surveys from 1968 through 1980, my analysis must be confined to those years. In each year I use the absolute value of the difference between a respondent's position and the respondent's perception of the candidate's position. Absolute values are used because I am concerned not with the direction of the distance between the respondent and the candidates, but with the gap itself. In each of the four elections, I measured the gap between the respondent's positions and the perceived positions of the major candidates: Nixon, Humphrey, and Wallace in 1968; Nixon and McGovern in 1972; Ford and Carter in 1976; and Reagan, Carter, and Anderson in 1980.

Indifference. I expect the situation most conducive to voting to occur when one candidate's positions are close to one's own position and all other candidates' are distant from it. To operationalize this notion, I took the absolute value of the difference between the respondent's position on a particular issue and the respondent's perception of each particular candidate's position on that same issue (*pdD*, *pdR*, and *pdM* for the absolute value of the perceived distance from the Democratic, Republican, and minor party candidates). Thus, in 1968 there were six measures: the absolute distance between the respondent and 1) Nixon on the Vietnam War and on urban unrest; 2) Humphrey on the Vietnam War and on urban unrest; and 3) Wallace on the Vietnam War and on urban unrest. In 1972, there were eight measures; that is, the absolute distance from the respondent and both Nixon and McGovern on each of four issues: liberalism, jobs, minority

rights, and equality for women. Similar measures were obtained for the same four issues in 1976 and 1980, with the latter including distance from Anderson's position.

To construct a measure indicating whether one was close to or distant from any or all candidates, I proceeded as follows. In 1972 and 1976—when there were only two major candidates—I took the absolute value of the difference between the respondent's and Democratic candidate's positions (*pdD*) and the respondent's and Republican candidate's positions (*pdR*) on each issue and reversed the scoring ($((|pdD - pdR| \times -1) + 7)$). Thus, if the respondent were close to one candidate and distant from the other, the respondent would get a low score on this measure for each particular issue. Similarly, if the respondent was close to or distant from both candidates, he would receive a high score. Thus, we would expect a negative relationship between scores on these indexes and voting: as the choice between candidates becomes less clear (i.e., as indifference increases), individuals should be less likely to vote.

I wanted to retain the same scoring and scale for the 1968 and 1980 elections, and to do so I had to adjust for the presence of a third candidate. In each election I made contrasts for each issue similar to the ones described immediately above. To illustrate this process for 1968, I created three new terms: *RD* = the absolute value difference between the respondent and Nixon (*pdR*) and the respondent and Humphrey (*pdM*); *RM* = the respondent and Nixon (*pdR*) vs. the respondent and Wallace (*pdM*); and *DM* = the respondent and Humphrey (*pdD*) vs. respondent and Wallace (*pdM*). I then subtracted the smallest of these new terms from the largest and reversed the scoring (as above). This allowed me to capture how clear one's choice was: if there is little difference between the largest and smallest of these measures (*RD*, *RM*, *DM*), then the respondent is indifferent. If, on the other hand, there is a big gap, the respondent is not indifferent. For example, assume that a respondent is 0 units from Humphrey, 5 units from Nixon, and 6 units from Wallace. From these scores it is obvious that the respondent has a clear choice (Humphrey) and is therefore not indifferent, and my measure of indifference must apprehend this. In this case, *RD* is $|5 - 0| = 5$; *RM* is $|5 - 6| = 1$; and *DM* is $|6 - 0| = 6$. Indifference would be measured as the largest of these (*DM*) minus the smallest (*RM*), with the scoring reversed: $((6 - 1) \times -1) + 7 = 2$. Thus, the respondent would have a very clear choice, as the only clearer choice would be indicated by a score of 1, and my measure of indifference reflects accurately the respondent's perceived choices. To reiterate: voting should be negatively related to indifference.

²In a somewhat similar vein, Kelley and Mirer (1974) used both partisanship and affect toward candidates and parties to predict (primarily) the direction of voting. Although they also looked briefly at nonvoting, they acknowledged that there was an important difference between studying decisions of whether to vote and the direction of the vote. "A comparison of candidates resulting in a choice among them should be one consideration—but not the only one—in the decision about whether to vote" (Kelley & Mirer, 1974, p. 574). I agree with them and include some of these other factors in my model (e.g., political efficacy, political interest, whether one was mobilized to vote).

Alienation

The measure of alienation is more straightforward. The concept of alienation is similar to the situation described above: respondents are distant from all the candidates on the perceived distance measures (*pdD*, *pdR*, *pdM*). I have operationalized alienation as the minimum of the absolute values of the perceived distance measures on each of the items.³ A low score on these minimum distance variables—or alienation—indicates that the respondent is close to at least one candidate (i.e., the respondent is not alienated), whereas a high score means that one is not close to any candidate (i.e., alienated). Thus, the alienation variables also should be negatively related to voting: as alienation increases, the likelihood of voting should decrease.

In sum, the lack of representation can lead to nonvoting through the absence of a clear choice (indifference, as measured by the joint distance variables), through the lack of any candidate close to one's positions (alienation, as indicated by the minimum distance measures), or through some combination of both.

Unlike earlier researchers (e.g., Brody & Page, 1973), I developed an extensive model of factors thought to explain voting and only considered the impact of indifference and alienation net of these other factors. This approach should help to guard against reaching a spurious interpretation of the relationship between interests and voting. All of these other independent variables and their coding are listed in Table 1. For each of them, missing data were recoded to the mean. Because I am only assessing the impact of the distance variables after controlling for age, sex, race, region of the country, education, occupation, income, whether one was contacted by the parties, political efficacy,⁴ political interest,⁵ and party identification, these will be conservative tests of my hypothesis that having one's interests represented increases the probability that one will vote.

Before continuing, it is important to note the

³The measure of indifference does not distinguish between two different reasons for nonvoting: being indifferent because one is equally close to all candidates and being indifferent because one is equally distant from them all. Alienation addresses this in part, as it captures whether one is close to any candidate. I thank an anonymous reviewer for bringing this to my attention.

⁴I used two items to measure external political efficacy (e.g., Abramson & Aldrich, 1982): 1) "People like me don't have any say about what the government does," and 2) "I don't think that public officials care much about what people like me think."

⁵Political interest is a measure of the respondent's interest in the political campaign.

limitations of these measures. First and most serious is the direction of causal flow between the perceived distance measures and voting. I have assumed that the perceived distance variables precede voting causally, but it is plausible that the reverse is true. Justification for this reasoning can be found in cognitive dissonance theory under the rubric of "post-decisional conflict reduction" (e.g., Brehm, 1956; Crawford, 1976). Basically this term describes a process in which once a choice is made among various options, the subjects tend to evaluate their chosen option much more positively than they did before making the choice, with the reverse being true for the options that were not chosen. Thus, in a postelection interview—in which voting decisions already have been made—it is possible that respondents first decided whether or not to vote and then rationalized whether they were close to any of the candidates.

Although this is a realistic possibility, I feel that the predominant causal flow is from attitudes to behavior. I base this judgment on the work of Brody and Page (1973). In their study of the 1968 election, they tested this notion by comparing pre-election and postelection candidate evaluations. Even though they found some evidence of post-decisional rationalization, they concluded that it "was not sufficiently prevalent" to warrant a reversal of causal attribution.⁶ However, because some rationalization does occur, the conclusions need to be tempered by this fact, and the reader should bear this in mind.

Second, there is no guarantee that the issues that have been measured are salient and ones upon which voting decisions are made.⁷ Given these and the fact that the major party candidates often do not differ very much from each other (e.g., Page, 1978), it may be better to view our analyses as suggestive.⁸

⁶What ideally would be needed is a comparison of the pre- and post-election evaluations of those who were undecided immediately before making the decision to vote or not to vote. According to my approach, these voters with no clear choice should not vote and should still have no clear choice after the election. Should they vote and have a clear choice after the election, this would indicate the presence of post-decisional conflict reduction.

⁷In 1968, questions were asked concerning the importance of these particular issues in the respondent's voting decision. These results indicate that these two issues were of substantial importance to the respondents.

⁸Fiorina (1981, pp. 139-143) pointed out that measures such as those which ask one to compare someone else's position with one's own position are subject to a psychological process in which one rationalizes the other's position as being near to one's own. Although

Table 1. Independent Variables Used in the Analyses

Variable	Description	Coding
Middle aged	R's age	1 = 35-54, 0 = All else ^a
Older aged	R's age	1 = 55+, 0 = All else
Sex	R's sex	1 = Male, 0 = Female
Race	R's race	1 = White, 0 = Nonwhite
South	Region where R lives	1 = South, 0 = Nonsouth
Some high school education	R's education level	1 = 9-11 yrs., 0 = All else ^b
High school graduate	R's educational level	1 = 12 yrs., 0 = All else
Some college education	R's educational level	1 = 13-15 yrs., 0 = All else
College graduate	R's educational level	1 = 16+ yrs., 0 = All else
Professional-technical	Occupation of head of household	1 = Professional, technical jobs, 0 = All else ^c
Clerical-sales	Occupation of head of household	1 = Clerical, sales job, 0 = All else
Blue collar	Occupation of head of household	1 = Blue collar job, 0 = All else
Family income	R's family income	In dollars
Contact	Whether R was contacted	1 = Contacted, 0 = Not contacted
Political efficacy	R's feelings of political efficacy	0-2; lowest to highest
Political interest	R's interest in politics	1-5; Not interested to Very interested
Democratic ID	R's party identification	1 = Democrat, 0 = All else ^d
Republican ID	R's party identification	1 = Republican, 0 = All else
Indifference: Liberalism	R's joint distance from all major candidates on the liberalism scale.	1-7, with 1=R close to or distant from all candidates, and 7=R close to one and distant from all other candidates.
Indifference: Jobs	R's joint distance from all major candidates on the jobs question.	Same as above
Indifference: Minority rights	R's joint distance from all major candidates on the minority rights question.	Same as above
Indifference: Women	R's joint distance from all major candidates on the women question.	Same as above
Indifference: Urban	R's joint distance from all major candidates on the urban question.	Same as above
Indifference: Vietnam	R's joint distance from all major candidates on the Vietnam question.	Same as above
Alienation: Liberalism	Minimum of the absolute value of R's perceived distance from any of the candidates on the liberalism scale.	1-7, with 1=R close to at least one candidate and 7=R distant from all candidates.
Alienation: Jobs	Minimum of the absolute value of R's perceived distance from any of the candidates on the jobs question.	Same as above
Alienation: Minority rights	Minimum of the absolute value of R's perceived distance from any of the candidates on the minority rights question.	Same as above
Alienation: Women	Minimum of the absolute value of R's perceived distance from any of the candidates on the women question.	Same as above
Alienation: Urban	Minimum of the absolute value of R's perceived distance from any of the candidates on the urban question.	Same as above
Alienation: Vietnam	Minimum of the absolute value of R's perceived distance from any of the candidates on the Vietnam question.	Same as above

^aOmitted category is young—age 21-34.^bOmitted category is 8 or fewer years of education.^cOmitted category is managerial jobs.^dOmitted category is independent, other or no party identification.

Results

The dependent variable is a dichotomous, vote-nonvote one with voting coded 1 and nonvoting coded 0. Because there are problems with the use of ordinary least squares regression with dichotomous dependent variables, I have chosen to use logistic regression. Good discussions of the problems of OLS with dichotomous variables and the benefits of logit analysis can be found in Dwyer (1983) and Hanushek and Jackson (1977).

The results of the logit analyses are shown in Table 2. Because I am interested only in the distance variables, I discuss only their effects. Furthermore, because I have no *a priori* reason to expect any of these distance variables to be more important than the others and because they are positively correlated with each other, I am more concerned with the performance of each set of them rather than any particular one.

For this reason, and because there is no clear rationale for the order in which the alienation and the indifference variables should be entered into the equations relative to each other, I chose to present the results of all the possibilities. I entered the alienation variables first, the indifference ones first, and both sets together. In order to determine whether or not the lack of representation is related to voting, I compared the increments in chi-square associated with including both the alienation and the indifference factors.⁹ I also examined the chi-square increments of alienation alone and alienation net of indifference, and vice versa. Taken together these comparisons should enable us to assess 1) if accounting for whether or not one's interests are represented increases our ability to explain nonvoting, and 2) if this is the result of indifference, alienation, or both.

In all four elections, the alienation and indifference variables when considered together significantly increase our ability to explain nonvoting (see Table 2). In 1968 this is primarily the result of the effects of indifference to the Vietnam

War (-.16), in 1972 to indifference to liberalism (-.15), in 1976 to indifference on the role of women (.19), and finally in 1980 to alienation on the jobs question (.19). At the most general level, then, this constitutes initial evidence that the lack of representation increases nonvoting.

One still needs to consider the separate impacts of alienation and indifference. Turning first to alienation, as a set the alienation variables alone significantly increase the explanation of nonvoting in 1972 (chi-square increment = 22.62 with 4 *df*), in 1976 (chi-square increment = 9.78 with 4 *df*), and in 1980 (chi-square increment = 14.14 with 4 *df*). In 1972 the lack of representation of liberalism (-.30) and minority rights (-.14) is primarily responsible, in 1976 liberalism (-.23) again is significant, whereas in 1980 it is because of alienation on the jobs question (-.21). Finally, in all elections the direction of the significant coefficients is as expected: increases in alienation are associated with decreases in voting.

Accounting for indifference significantly enhances our understanding of voting in 1968 (chi-square increment = 9.60 with 2 *df*), in 1972 (chi-square increment = 33.79 with 4 *df*), and in 1976 (chi-square increment = 16.69 with 4 *df*). In 1968, it results primarily from the joint distance between the respondents' and the candidates' views on the Vietnam War (-.18), with the gap between the respondents' and the candidates' views on liberalism (-.20) and on minority rights (-.10) statistically significant in 1972, and liberalism (-.14) and an equal role for women (.19) significantly related to voting in 1976. Further, except for the women issue in 1976, the direction of the coefficients is as expected: as indifference increases, voting decreases.¹⁰

From the above, it is clear that both indifference and alienation lead to nonvoting. To determine better which factor has a greater impact, I have undertaken two procedures. First, I examined the increments in chi-square for each of the two sets of distance variables net of the other set. The results of these comparisons (also listed in Table 2) indicate that, net of indifference, aliena-

this is a problem, if such a process is operating in these data, it will result in having most people report being closer to the candidates than they actually are. This will attenuate the relationships between these measures and voting, and thus the results may underestimate the effects of having one's interests represented on the likelihood of voting.

⁹Testing for the significance of a set of variables involves comparing the chi-square value obtained from a model with those variables to the chi-square value obtained from a nested model excluding that set of variables. If there is a significant increment in chi-square when the set is included, then that set of variables has significantly increased our explanatory power.

¹⁰As an anonymous reviewer brought to our attention, failure to answer the candidate placement questions could lead to nonvoting if this failure indicated that the respondent was unable to perceive a candidate's position. I checked for this possibility by creating new variables representing missing-nonmissing on each of the self- and candidate-placement items. As a set, these variables were not significant in 1968 and 1976, and in the other two elections their significance is mainly due to the respondents' failing to answer the self-placement questions (these respondents were then not asked the candidate placement questions). Thus, this explanation largely is not valid.

Table 2. Logistic Regression Coefficients for Voting, 1968-1980

	1968			1972			1976			1980		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Middle aged	.92*	.92*	.92*	.67*	.64*	.66*	.99*	.95*	.96*	.78*	.74*	.76*
Older aged	1.24*	1.24*	1.24*	.80*	.74*	.76*	1.01*	.97*	.96*	1.23*	1.17*	1.22*
Sex	.19	.21	.21	.15	.15	.14	.37*	.32*	.32*	.11	.07	.06
Race	.09	.24	.21	-.17	-.08	-.08	-.11	-.05	-.06	-.09	-.03	-.07
South	-.38*	-.35*	-.36*	-.46*	-.54*	-.51*	-.45*	-.44*	-.44*	-.24	-.25	-.24
Some high school education	.31	.31	.30	.20	.15	.16	.17	.14	.14	.00	.03	.02
High school graduate	.97*	.98*	.98*	.76*	.74*	.74*	.63*	.59*	.58*	.74*	.71	.74*
Some college education	.52	.52	.52	1.12*	1.04*	1.04*	1.26*	1.22*	1.20*	.68*	.67*	.66*
College graduate	.72*	.71	.71	1.15*	1.10*	1.06*	.98*	.92*	.91*	1.67*	1.71*	1.66*
Professional-technical	.32	.30	.32	.22	.22	.22	.46	.48	.48	-.28	-.27	-.28
Clerical-sales	.41	.46	.44	.20	.16	.18	.05	.06	.07	-.01	-.02	-.01
Blue collar	-.29	-.30	-.30	.05	.05	.05	.04	.06	.07	-.17	-.18	-.16
Family income ^a	.64*	.66*	.66*	.45*	.44*	.44*	.29*	.30*	.28*	.10	.08	.09
Contact	.56*	.54	.55*	.76*	.74*	.74*	.71*	.73*	.74*	.71*	.68*	.70*
Political efficacy	.41*	.45*	.44*	.27*	.30*	.29*	.30*	.31*	.30*	.48*	.49*	.48*
Political interest	.34*	.33*	.32*	.32*	.30*	.30*	.33*	.33*	.33*	.42*	.43*	.42*
Democratic ID	.63*	.63*	.63*	.61*	.62*	.62*	.42*	.39*	.40*	.74*	.71*	.74*
Republican ID	.57*	.57*	.56*	.79*	.78*	.78*	.56*	.56*	.55*	.83*	.78*	.80*
Alienation: Urban	-.07		-.05									
Alienation: Vietnam	-.14		-.09									
Indifference: Urban		-.04	-.02									
Indifference: Vietnam		-.18*	-.16*									
Alienation: Liberalism												
Alienation: Jobs				-.39*		-.16	-.23*		-.17	-.20		-.12
Alienation: Minority rights				.00		.04	.03		.07	-.21*		-.19*
Alienation: Women				-.14*		-.12*	-.06		-.04	-.14		-.15
Indifference: Liberalism				-.02		.03	-.02		-.06	-.01		-.03
Indifference: Jobs					-.20*	-.15*		-.14*	-.08		-.14	-.08
Indifference: Minority rights					-.06	-.08		-.06	-.07		-.10	-.06
Indifference: Women					-.10*	-.08		-.04	-.03		-.01	.02
Constant					-.01	-.02		.19*	.19*		.07	-.07
Chi-square	-2.22	-3.36	-2.96	-1.30	-2.99	-2.46	-1.79	-2.56	-2.00	-1.53	-2.95	-1.70
df	335.78*	340.67*	342.53*	514.91*	526.08*	534.52*	446.23*	453.14*	458.54*	391.63*	394.37*	401.11*
Basic Model Chi-square ^b	331.07*			492.29*		26	22	22	26	22	22	26
df	18			18		26	22	22	26	22	22	26

Table 2 (continued)

	1968			1972			1976			1980		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Increment in Chi-square for: Alienation and indifference <i>df</i>	11.46*			42.23*			22.09*			16.13*		
	4			8			8			8		
Alienation	4.71			22.62*			9.78*			14.14*		
Alienation net of indifference	1.86			8.44			5.40			9.48†		
Indifference	9.60*			33.79*			16.69*			6.65		
Indifference, net of alienation <i>df</i>	6.75*			19.61*			12.31*			1.99		
	2			4			4			4		

^aMultiply coefficient by .0001.
^bAll independent variables, except for alienation and indifference.
**p* < .05.
†*p* approximately .055.

tion never produces a significant increment in chi-square, whereas accounting for indifference significantly augments our understanding of non-voting—net of alienation—in 1968 (chi-square increment = 6.75 with 2 *df*), in 1972 (chi-square increment = 19.61 with 4 *df*), and in 1976 (chi-square increment = 12.31 with 4 *df*). Thus, it appears at this point that indifference is more important than alienation.

I have approached this issue in a second way. Logistic regression coefficients, unlike OLS coefficients, do not have a constant effect on the dependent variable across all values of the independent variables, so the effect of one independent variable must be evaluated at its different values and at certain values of all other independent variables. It is conventional to make these evaluations at the means of the other independent variables. Thus, to provide further insight on the impacts of alienation and indifference, I have calculated the probabilities of voting for the alienation and indifference variables, holding all other variables—including whichever set of distance variables is not being evaluated—at their respective means (see Table 3). I can illustrate how I arrived at these figures by using alienation in 1968 as an example. (The same procedure was used in all years and also for indifference.) I first multiplied the logistic coefficient of each of the independent variables listed in Table 3, column 3, except for the alienation measures, by the mean of the respective variable. For each alienation measure, I multiplied the logistic coefficient by the respondent's actual score on the respective variable. Thus, each respondent was given the mean value on all the independent variables except for alienation, on which they received their actual value. After summing these together along with the intercept and terming this new quantity *XB*, I calculated the probability of voting as equal to: $1/(1 + \text{exponent}(-XB))$. This gives the probability of voting across levels of alienation, with every other factor held at its mean. Because all variables, except for alienation, are fixed at their means, all the variance in the probability of voting results from alienation (or likewise from indif-

ference, when I calculate the results for it). Thus, the standard deviations of these probabilities supply information about the relative effects of alienation and indifference.

These results, shown in Table 3, indicate that in all but 1980 the standard deviations of indifference are considerably larger than those of alienation.¹¹ This difference in standard deviations can be interpreted to signify that there is greater variability in the likelihood of voting across levels of indifference than across levels of alienation. To illustrate, let us assume that being one standard deviation above or below the mean indicates that one is very high or very low on a particular measure. Given this, in 1968 the difference in the probability of voting between a highly indifferent (.7825) and not indifferent person (.8511) was almost 7%. The same comparison for an unalienated (.8241) and an alienated individual (.7923) produced a difference of slightly more than 3%. Similarly, the differences between those who were one standard deviation above and one standard deviation below the mean were 9% for indifference and 6% for alienation in 1972, and 8% for indifference and 5% for alienation in 1976. Taken together, these results confirm that both alienation and indifference lead to non-voting, but that indifference has a greater impact.

Summary and Discussion

The most important finding is that measuring the gap between an individual's positions on issues and his perceptions of all the candidates' positions on the same issues, net of a host of factors usually invoked to explain voting, significantly increases one's ability to explain turnout. In general, if one has a clear choice among the candidates (is not indifferent) and one's policy preferences are close to at least one candidate (one is not alienated), one is much more likely to vote. Furthermore, although both alienation and indif-

¹¹The means are approximately equal throughout, as they should be.

Table 3. Probabilities of Voting by Alienation and Indifference, 1968-1980^a

	Alienation				Indifference			
	$\bar{X}$	SD	+1SD	-1SD	$\bar{X}$	SD	+1SD	-1SD
1968	.8082	.0159	.7923	.8241	.8168	.0343	.7825	.8511
1972	.7835	.0297	.7538	.8132	.7712	.0449	.7263	.8161
1976	.7829	.0269	.7560	.8098	.7849	.0392	.7457	.8241
1980	.7652	.0447	.7205	.8099	.7757	.0253	.7504	.8010

^aAll other variables are evaluated at their means.

ference can lead to nonvoting, being indifferent to the candidates has a greater impact on nonvoting than does being alienated from them. The fact that these results were obtained net of an extensive set of factors related to voting in different elections provides additional evidence of their existence and persistence.

There are several implications of these results. First, this research was prompted by a concern that the options available to individuals generally have been neglected in explaining nonvoting in the United States. The extant literature focuses almost solely on individualistic characteristics, ignoring the interplay between the attributes of individuals and the choices presented to them. For example, in his study of turnout Cavanagh (1981, p. 63) contended that "the current rightward drift in policy initiatives may owe its source to the contemporaneous class shift in participation patterns." Thus Cavanagh blames the victim: if more lower class people had participated, the current rightward shift might not have occurred. His interpretation assumes that the politics of the elite faithfully mirror those of the mass electorate. Although this assumption is central to responsive democratic government, research in both the United States (e.g., Hamilton, 1972) and other countries (see Zipp, 1978, for a summary) calls it into question. Although we know that various individual-level factors are associated with nonvoting (see Table 2) and cannot be ignored, I am suggesting that the fit between individuals and the options presented to them also needs to be incorporated into explanations of nonvoting.

These results have a second implication for individualistic approaches to explaining nonvoting. Another way of saying that the lack of a representative candidate decreases voting is that nonvoting may be the outcome of a reasoned, thoughtful political position. However, recent research on nonvoting tends to treat it as the failure to act politically rather than as a chosen form of political action. The difference between these two is quite important. For example, treating nonvoting as a failure to act, Brody (1978), Cassel and Hill (1981), Cavanagh (1981), Reiter (1979), and Schaffer (1981) all predicted that increased levels of education from 1960 to 1980 would result in increased levels of voting, because education increases the awareness of the needs for and the benefits of voting. The underlying assumption is clear: nonvoting stems from a lack of education or knowledge, not from an intelligent, reasoned position. Thus, the solution to the problem of nonvoting is to educate the nonvoters. However, taking the perspective that nonvoting may be a thoughtful, chosen stance directs one away from educating the nonvoter and toward an examination of the factors that cause nonvoting

to be a legitimate alternative way of expressing political preferences. In this article I have contended that one of these reasons is that individuals do not have their interests represented in the political sphere. For some citizens, nonvoting may be a failure to act politically, but for others it is a chosen form of political action. Neither reason for nonvoting can be treated as the other.

There is one approach to explaining nonvoting with which my results generally are quite consistent: rational choice theory and the idea of rational abstention (e.g., Brody & Page, 1973; Davis, Hinich, & Ordeshook, 1970; Downs, 1957; Ordeshook, 1970). At the risk of oversimplification, this view argues that, given the costs of voting, if the outcome does not make any difference, the rational citizen does not vote. Indeed I have shown that not having a clear choice among the candidates (i.e., not having a preferred outcome) leads to nonvoting, and thus these results can be used to support this notion of rational choice theory.

However, as noted above, this research was not motivated by seeking to test the utility of Downsian spatial models. Rather, I was influenced most by a concern for the lack of options that exist for many individuals,¹² and because I have established that the absence of representation leads to nonvoting, it is appropriate for scholars to ask why these options do not exist for many citizens. Although a thorough examination of this question is beyond the scope of this article, a brief outline is suggested. Single district, single member, "first-past-the-post" presidential electoral systems encourage the development of two centrist, brokerage-style parties and weaken support for third parties. Combined with historical and other forces (e.g., American Political Science Association, 1950), such rules have resulted in a less-than-responsible two-party system in which the parties do not provide coherent and distinct ideologies and programs. This system negates even the requisites of the "competitive theory of democracy" and elevates nonvoting to the level of a third choice. In addition, several current factors create a considerable distance between candidates and most citizens: the upper-middle-class backgrounds of most candidates; the need of candidates to have or to be able to attract the large sums of money required to conduct viable cam-

¹²Looking only at the marginals for the indifference variables, for 10 of the 14 variables more than 50% of the respondents are in the two lowest categories—i.e., the "least representation" categories. In two of the remaining four, more than 40% are in these categories. Thus, it is safe to say that these options do not exist for a good number of citizens.

paings; the importance of the media and their almost complete neglect of third-party candidates; and, perhaps owing to the increased role of the judiciary and other unelected officials (Burnham, 1967), the belief on the parts of some that elected candidates cannot really make good on their promises (e.g., Kimball, 1972; Parenti, 1980, especially chap. 11).

In conclusion, I feel that it would be more fruitful intellectually if scholars shifted their focus away from explaining nonvoting primarily by noting the deficiencies of nonvoters to one that also includes an understanding of the circumstances that lead some individuals to use nonvoting as a reflection of political beliefs.

Appendix:

Candidate Distance Questions, 1968-1980

(1) Urban (1968 only)

There is much discussion today about the best way to deal with the problem of urban unrest and rioting. Some say it is more important to use all available force to maintain law and order—no matter what the results. Others say it is more important to correct the problems of poverty and unemployment that give rise to the disturbances. And, of course, other people stress opinions in between. Suppose the people who stress the use of force are at one end of this scale—at point number 7. And suppose the people who stress doing more about the problems of poverty and unemployment are at the other end—point number 1. Where would you place (yourself, Humphrey, Nixon) on this scale?

(2) Vietnam (1968 only)

There is much talk about “hawks” and “doves” in connection with Vietnam and considerable disagreement as to what action the United States should take in Vietnam. Some people think that we should do everything necessary to win a complete military victory, no matter what results. Some people think we should withdraw completely from Vietnam right now, no matter what results. And, of course, other people have opinions somewhere between these two extreme positions. Suppose the people who support an immediate withdrawal are at one end of this scale at point number 1. And suppose people who support a complete military victory are at the other end of the scale at point number 7. At what point would you place (yourself, Humphrey, Nixon)?

(3) Liberalism (1972-1980)

We hear a lot of talk these days about liberals

and conservatives. I'm going to show you a 7-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about it?

(4) Jobs (1972-1980)^a

Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Suppose these people are at one end of this scale—at point number 1. Others think that the government should just let each person get ahead on his own. Suppose that these people are at the other end—at point number 7. And, of course, some people have opinions in between. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about this?

(5) Minority Rights^a (1972-1980)

Some people feel that the government in Washington should make every possible effort to improve the social and economic position of blacks and other minority groups. Others feel that the government should not make any special effort to help minorities because they should help themselves. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about it?

(6) Women (1972-1980)

Recently there's been a lot of talk about women's rights. Some people feel that women should have an equal role with men in running business, industry, and government. Others feel that a woman's place is in the home. Where would you place (yourself, the Democrats, the Republicans) on this scale, or haven't you thought much about it?

^aThere were slight changes in wording from 1972 to 1980, but only in the way in which the respondent was informed about the ends of the scale. In all cases, the options were the same, as was the substance of each question.

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Voter Participation and Strategic Uncertainty

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The paradox of not voting is examined in a model where voters have uncertainty about the preferences and costs of other voters. In game-theoretic models of voter participation under complete information, equilibrium outcomes can have substantial turnout even when voting costs are relatively high. In contrast, when uncertainty about preferences and costs is present, only voters with negligible or negative net voting costs participate when the electorate is large.

This article provides a unified treatment of two areas of longstanding theoretical attention in political science. The first is the paradox of not voting as addressed by Downs (1957), Tullock (1967), and Riker and Ordeshook (1968). Why does anyone bother to vote, given that voting is presumably costly and that the probability that one's vote will affect the outcome is presumably small? The second area is the role played by uncertainty or the lack of information in affecting political processes.

Uncertainty and the Paradox of Not Voting

There are three types of uncertainty that are potentially germane to the paradox of not voting. The first type, uncertainty over alternative outcomes, has attracted substantial attention in the theory of competitive elections, beginning with Shepsle (1972). Just what would be the policies of a candidate if he or she were elected? In the strategic interaction among voters, this form of uncertainty is of no consequence. The randomness occurs at the end of the game tree and, in the context of a game-theoretic model, each citizen will simply replace the final lottery with its expected utility value. In contrast, the second and third types involve strategic uncertainty.

The second type results from voters not knowing whether other citizens will vote or abstain. Because there is no compulsion to reveal one's decision before the poll, the appropriate game-theoretic model is one in which all citizens make decisions simultaneously. In terms of a game tree, the presence of simultaneous play implies that players do not see the results of all prior or contemporaneous moves. Consequently, voter participation games are games of imperfect information.

The third type of uncertainty recognizes that any one voter is generally poorly informed about the voting costs and preferences of other voters, which leads to a game of incomplete information. In terms of classical game theory, this uncertainty can be modeled by having "nature" move at the beginning of the game, assigning costs and preferences to the voters. Each voter, however, is permitted to observe only his or her own costs and preferences. Thus, one's own costs and preferences remain private information. Once nature has moved, the game advances to a branch corresponding to a game of complete (but imperfect) information with the costs and preferences assigned by nature. Voters then move in this game but with an additional source of uncertainty; not only do they not know the moves of other citizens, they also do not know the characteristics (costs and preferences for political alternatives) of these citizens.

Model building in political science has generally focused on strategic uncertainty regarding moves while ignoring uncertainty regarding player characteristics. When this latter type of uncertainty is absent, the appropriate form of analysis is to ascertain Nash equilibria. Nash equilibrium analysis has been most prominent in the analysis of spatial competition by candidates. In simple cases, like the classic Hotelling-Black median voter theorem, pure strategy equilibria result, but in more general situations there is equilibrium

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only in mixed strategies (McKelvey & Ordeshook, 1976).¹

The Bayesian Nash Equilibrium Concept

When we add uncertainty about player characteristics, the strategies available to the players change in a fundamental way. Whereas in a voter participation game with complete information strategies are limited to vote or abstain, in the game of incomplete information, strategies take the form, "If my characteristics assigned by nature are so and so, vote; otherwise, abstain." Put somewhat differently, analysis takes place in a framework where each player has a decision rule that maps his own characteristic into a specific action (vote or abstain). The decision rule chosen by a voter will depend upon his or her beliefs about both the decision rules that other voters will use and the distribution of other voters' characteristics. In equilibrium, all the decision rules and beliefs must be consistent in the sense that 1) these beliefs are correct, and 2) given the rules adopted by the other players, no single player will wish to alter his own decision rule. The equilibrium characterized in this way in fact represents a Nash equilibrium to the expanded game where "nature" moves first and assigns player characteristics; this is commonly referred to as a Bayesian equilibrium. In addition to analyzing Bayesian equilibrium in general voter participation games, we also present a simple example that illustrates geometrically the relationship between Bayesian and Nash equilibria in these games.

We believe the Bayesian equilibrium approach has many potential applications in political science beyond this investigation of the paradox of not voting. For example, there is also substantial uncertainty in legislative and bureaucratic settings. In turn, this uncertainty may, as the analysis below indicates, strongly affect our theoretical conclusions about these political processes.

Alternative Models of Voter Participation Games

Our analysis considers voter participation games in the context of two alternative plurality elections, as found in the general literature on the paradox of not voting. Following the literature, we use the simplification of a utility function that

is additive between instrumental benefits and the fixed net cost of voting. This simplification leads to the well-known equation:

$$R = pB - C + D, \quad (1)$$

where R is expected net benefits of voting, p is the probability of casting a decisive vote, B is the difference in benefits engendered by the voter's more preferred alternative winning over his less preferred alternative, C is a fixed voting cost (such as the time spent in going to and from the polls), and D is a fixed benefit which derives from the act of voting, such as a sense of citizen duty or the avoidance of sanctions imposed for not voting. We combine the last two terms and refer to $c = C - D$ as the fixed net cost of voting. Additivity conveniently implies that all the information each voter requires about other voters can be summarized by two pieces of information. First, it is necessary to have prior information of how many other voters prefer each of the two alternatives. Second, prior information is needed only about the ratio of costs to benefits of each other voter and not about the joint distribution of both costs and benefits. That is, without loss of generality, we can, given additivity, normalize equation (1) such that $B = 1$ for all voters.

It has been argued² that with large numbers of voters, p is presumably very close to 0, so if D is less than C , rational voters will abstain since $R = p - c < 0$. Of course, if this is true for everyone and if everyone follows this logic, then $p = 1$. One proposed solution to this "paradox of not voting" (Downs, 1957) is that substantial turnout occurs despite $p \cong 0$, because citizens are imbued with a sense of citizen duty so that $D > C$. However, many theorists considered the paradox unresolved because they were unwilling to accept that voting generally led to net benefits rather than net costs, even though mass voting is widespread. In fact, substantial turnout is found not only in some national elections, like U.S. presidential elections, which are not strictly single issue, two-alternative ballots, but also in other contexts, such as the French presidential runoff, where the institutional setup implicit in equation (1) is in fact present.

An initial attempt to resolve the paradox was made by Ferejohn and Fiorina (1974), who simply asserted that voters were not expected utility maximizers but regret minimaxers. Their approach met considerable resistance.³ Moreover,

¹These games may be contrasted with chess and other games of perfect information which have sequential moves and before any move all previous moves are known to all players. In such games, there is no strategic uncertainty, and nontrivial mixed strategies never constitute equilibria.

²See, for example, Meehl (1977) and Chamberlain and Rothschild (1981).

³See Beck (1975), Mayer and Good (1975), Stephens (1975), Tullock (1975), and Thompson (1982), for discussions of the application of the minimax regret criterion to the voting decision.

the minimax regret hypothesis also seems inconsistent with empirical observations. For example, a regret minimaxer may vote regardless of whether the outcome is expected to be a tie or a 4-to-1 victory; the probability of casting a decisive vote is irrelevant under this decision criterion. Yet there is persistent observation of higher voting rates in closely contested elections.⁴

Rather than abandoning expected utility, more recent research has questioned the presumption that p is virtually zero. After all, equation (1) represents a purely decision-theoretic model if p is exogenously given. If we investigate how p is endogenously determined by the interaction of voters, we will perhaps find that p is indeed far from zero. With p far from zero, substantial turnout can result.

In earlier work, we indeed arrived at the conclusion that substantial turnout could occur (Palfrey & Rosenthal, 1983, 1984). There we assumed that every voter had complete information about the preferences and voting costs of every other voter. Then, for fixed alternatives (e.g., candidate positions), we modeled the voters as playing a non-cooperative game in which each voter has, essentially, two strategies: vote for the more preferred alternative or abstain. Surprisingly, we found that some of the Nash equilibria to this game had substantial turnout. In equilibrium, the presumably small p in equation (1) could in fact be 1 or very close to 1, independent of electorate size.

Although the full information model might have much to say about participation in committees or small groups, the full information assumption is clearly unrealistic for large electorates. In a pair of articles, Ledyard (1981, 1984) explored a more general model in which voters were uncertain about the voting costs and preferences of other voters. He showed that equilibria exist with positive turnout (as long as the alternatives are distinct), and that the condition of everyone abstaining is not an equilibrium. Consequently, some voting is to be expected. Beyond this, Ledyard is unable to obtain conclusive results about how much voting takes place in a voter equilibrium in large electorates.

In this article we demonstrate, within the framework of Ledyard's model, that in very large electorates the only voters are citizens with net positive benefits from the act of voting, citizens whose sense of duty outweighs any cost in voting. We have come full circle and are once again beset by the paradox of not voting.

It is important to emphasize, though, that the paradox requires both large electorates and sub-

stantial uncertainty. We can in fact have equilibrium with high turnout in large but extremely well-informed electorates or in small electorates subject to strategic uncertainty, even when voting costs are relatively large.

In the next section of the article we review the full information model. In our earlier development of this model (Palfrey & Rosenthal, 1983), we assumed all voters had identical costs. Finding high turnout in informed electorates is not dependent on this assumption. One can readily illustrate high turnout equilibria when costs are variable but known to all citizens. After the exposition of the full information model, we move half-way to the Ledyard model in the third section. Although we allow uncertainty regarding relative costs, we maintain full information regarding the number of voters preferring each of the two alternatives. We show that, in large electorates, even this partial degree of uncertainty is sufficient to cause voters with positive net voting costs to abstain in equilibrium. In the fourth section we add uncertainty regarding preferences and show that this does not change our basic results.

Voter Participation Games with Complete Information

There are N voters and two alternatives to vote for, alternative 1 and alternative 2. An election is held, and each voter has three choices: vote for 1; vote for 2; abstain.

If one alternative has more votes than the other, the alternative with the greater number wins. In the event of a tie, two rules are used in practice. First, one alternative is designated before the election as the winner in the event of a tie. Second, a fair coin is tossed to decide the winner. Our three main theorems hold for both rules and for a wide class of other tie-breaking procedures; for expositional clarity, we develop the theorems assuming the first, or status quo, rule. Although the first rule is simplest for general theorems, the second or coin-toss rule leads to a simple but important example. Thus, we henceforth use the first rule except when discussing this example.

Each voter is in one of two groups, T_1 or T_2 . There are N_1 voters in T_1 and N_2 in T_2 , with $N_1 + N_2 = N$. Voters in group T_1 prefer alternative 1, and voters in group T_2 prefer alternative 2. The cost of voting is denoted c_i for voter i . The payoff to a voter depends upon his cost, his preferences, which alternative wins, and whether or not he casts a vote. Denote the number of votes cast for alternative 1 by n_1 and the number of votes cast for alternative 2 by n_2 . Recalling that the benefits of winning and losing have been normalized to be 1 and 0, respectively, the possible

⁴See, for example, Rosenthal and Sen (1973) and Foster (1984).

Table 1. Payoff Matrix for Voters in T_1

	Vote	Abstain
$n_1 \geq n_2$	$1 - c_i$	1
$n_1 < n_2$	$-c_i$	0

payoffs to a voter in T_1 and T_2 are given in Tables 1 and 2.

The voting game is therefore parametrized by the number of voters in each group and the voters' (possibly different) voting costs. Because pure strategies fail to exist except in a few very special cases, we look to mixed-strategy equilibria where a mixed voting strategy is a probability of voting, which we denote by q_i for voter i . A well-known result from game theory guarantees existence of at least one mixed strategy equilibrium to our voter participation game. We define an equilibrium more precisely below:

$(q_1^*, \dots, q_N^*)$ is a *Mixed Strategy (Nash) Equilibrium (MSE)* if for all i :

- (A) $q_i^* > 0$ only if voting yields at least as high a payoff as nonvoting given $(q_1^*, \dots, q_{i-1}^*, q_{i+1}^*, \dots, q_N^*)$.
- (B) $q_i^* < 1$ only if nonvoting yields at least as high a payoff as voting, given $(q_1^*, \dots, q_{i-1}^*, q_{i+1}^*, \dots, q_N^*)$.
- (C) $q_i^* \in [0, 1]$.

One can immediately deduce from A and B that $0 < q_i^* < 1$ only if voter i is indifferent between voting and not voting, given everyone else's probabilities of voting. Thus we can give conditions that characterize mixed strategy equilibria. We denote n_1^i and n_2^i as the number of votes received in equilibrium by alternatives 1 and 2 if i does not participate. For voters in T_1 :

$$\begin{aligned}
 0 < q_i^* < 1 & \text{ only if } \text{Prob}(n_1^i \geq n_2^i) \\
 & = \text{Prob}(n_1^i \geq n_2^i - 1) - c_i \\
 & \text{ or } c_i = \text{Prob}(n_1^i = n_2^i - 1) \quad (2) \\
 0 = q_i^* & \text{ only if } c_i \geq \text{Prob}(n_1^i = n_2^i - 1) \quad (3) \\
 1 = q_i^* & \text{ only if } c_i \leq \text{Prob}(n_1^i = n_2^i - 1) \quad (4)
 \end{aligned}$$

Analogous conditions hold for voters in T_2 .

In Palfrey and Rosenthal (1983), equilibrium solutions are analyzed for the special case in which all voters have the same cost of voting, that is, $c_1 = \dots = c_N = c$. The numerous solutions can be divided into two categories; the first category, where all voters' probabilities of voting are strictly between 0 and 1 is called a *totally mixed strategy equilibrium* (TMSE); that is, no one is definitely voting or abstaining. Most of the analyzed solutions of this kind have the property that q_i^* approaches 0 for all voters as the number of voters becomes large. The other type of equilibrium has all voters in one group voting with some probability $q \in (0, 1)$, and the voters in the other group are divided into two subgroups, one in which voters definitely abstain and one in which voters definitely vote. This second kind of equilibrium does not have the property that voter turnout necessarily becomes very light as N becomes large. Rather, expected turnout may be as great as twice the size of the minority group (i.e., $2\min\{N_1, N_2\}$). Furthermore, even very small minorities can have a substantial chance of winning the election.

Although this result is very interesting because it suggests that substantial voting can occur when expected utility maximizing voters have relatively high voting costs and there is no citizen duty, the type of equilibrium that supports high turnout seems very fragile. In particular one side must be split into subgroups of voters and nonvoters in a precise way so that there is no uncertainty about how many votes one of the two alternatives will receive, because if there is too much uncertainty in equilibrium, then the probability that the election is close (i.e., either a tie or one vote away from a tie) approaches 0 as N gets large.

We also constructed one example of a totally mixed strategy equilibrium to a voting game where there was substantial voting even in arbitrarily large electorates. Because this example will be useful later on to help interpret equilibrium solutions with incomplete information, it is worthwhile to work through it here.

Example. Let all voters have the same cost $c \in (0, 1/2)$. Let $N_1 = N_2$. In order to make the problem completely symmetric, the tie-breaking rule

Table 2. Payoff Matrix for Voters in T_2

	Vote	Abstain
$n_1 \geq n_2$	$-c_i$	0
$n_1 < n_2$	$1 - c_i$	1

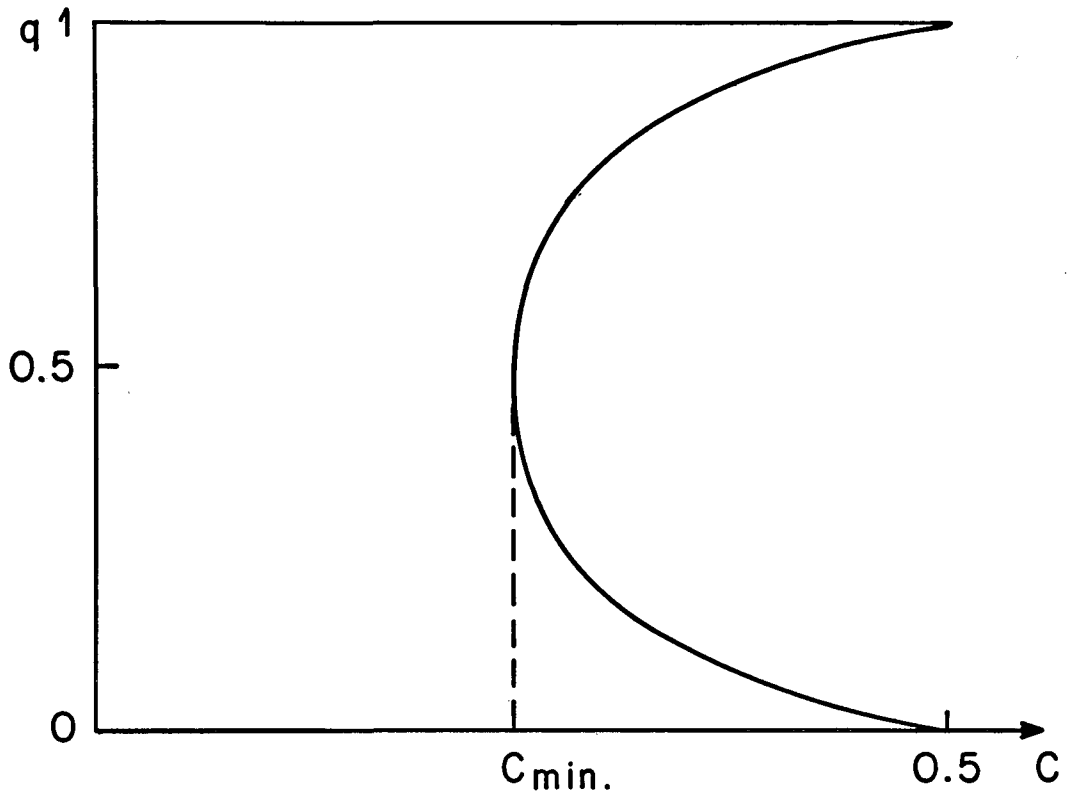


Figure 1. Totally Mixed Strategy Equilibrium Solutions as a Function of c with the Coin Flip Rule if $N_1 = N_2$

will be to toss a fair coin. Palfrey and Rosenthal (1983) show that any symmetric totally mixed strategy equilibria satisfy the following equation, which corresponds to equation (2), except for the difference resulting from the coin-toss tie-breaking procedure:

$$2c = \sum_{k=0}^{N_1-1} \binom{N_1-1}{k} \binom{N_1}{k} q^{2k} (1-q)^{2N_1-2k-1} + \sum_{k=0}^{N_1-1} \binom{N_1-1}{k} \binom{N_1}{k+1} q^{2k+1} (1-q)^{2N_1-2k-2}.$$

If c is between 0 and $1/2$, this equation has either 0, 1, or 2 solutions between 0 and 1. Figure 1 below illustrates the equilibrium solutions for each value of c between 0 and $1/2$, for a fixed value of N_1 .

As one can see from the figure, there exists no equilibrium of this type for $0 < c < c_{\min}$, exactly one equilibrium for $c = c_{\min}$ and exactly two

equilibria for $c_{\min} < c < 1/2$. As N_1 gets large, this figure changes in a systematic and interesting way. The lower bound, $c_{\min}$, converges to 0, and the "upper" and "lower" equilibria for $c > c_{\min}$ converge to 1 and 0, respectively. Therefore, in large electorates of this special sort, there will be exactly two symmetric totally mixed strategy equilibria for any cost between 0 and $1/2$: one with almost everyone voting and one with almost nobody voting.

This example of high turnout in large electorates has the unappealing feature that there is a second equilibrium with almost no one voting. Apparently the only reason the upper one can be sustained is that the two electorates are the same size, so that for q close to 1, the probability of a tied election is very high. Again, the result rests on the fact that in equilibrium, with a large electorate, there is essentially no strategic uncertainty. The next section examines the effects on equilibrium turnout of introducing an additional source of strategic uncertainty. The results presented below establish that the presence of strategic

uncertainty regarding costs and preferences eliminates all equilibria with high turnout in large electorates.

Voter Participation Games with Incomplete Information

Existence of Bayesian Equilibrium

Large turnout equilibria appear to be very fragile and seem to depend on there being very little uncertainty about the outcome in equilibrium. What these fragile equilibria depend on is that voters know with certainty what some subset of the voters are going to do (in particular the voters in the group who are playing pure strategies of voting and abstaining). It is this strategic certainty which permits p to be substantial in equilibrium even in very large electorates. In contrast to a game of complete information, a game with private information induces uncertainty about the behavior of all voters. In this section we consider a very simple kind of private information, namely that each voter knows his own voting cost (or ratio of cost to benefit), but has only probabilistic information about other voters' costs.³

In order to economize on notation, we will assume for voters in T_1 that each of their voting costs are independently and identically distributed according to a distribution function $F_1(c)$; for voters in T_2 , voting costs are independently and identically distributed according to a (possibly different) distribution function $F_2(c)$. In other words, the probability that any randomly selected voter in T_1 has a cost less than or equal to $\hat{c}$ is $F_1(\hat{c})$ and the probability that any randomly selected voter in T_2 has a voting cost less than or equal to $\hat{c}$ is $F_2(\hat{c})$. Each voter, however, knows her or his own actual voting cost, c_i , but has only the common probabilistic knowledge about others' voting costs. The other two parameters of the model, N_1 , N_2 , are commonly known by all voters.

The strategies in this type of game, a game of incomplete information, are defined differently from strategies in a game of complete information, because each voter i is aware that another voter's decision (vote or abstain) will depend on that other voter's actual voting cost, which voter i does not know. Therefore, each voter is presumed to have a decision rule, which specifies whether to vote or to abstain as a function of c_i . This condi-

tion results in two ranges of costs, one where i votes for his preferred candidate and another where i abstains. As will be apparent shortly, there is no loss in generality here in restricting attention to decision rules of the following form. Each voter i has a critical cost level, $\hat{c}_i \in [0, 1]$, and the decision rule of voter i is then to vote if $c_i < \hat{c}_i$ and abstain if $c_i \geq \hat{c}_i$.

The following additional notation is also useful. Let:

- $\hat{c}$ = $(\hat{c}_1, \dots, \hat{c}_n)$
- $\hat{c}_{-i}$ = $(\hat{c}_1, \dots, \hat{c}_{i-1}, \hat{c}_{i+1}, \dots, \hat{c}_n)$
- n_1 = the number of voters in T_1 for whom $c_i < \hat{c}_i$
- n_2 = the number of voters in T_2 for whom $c_i < \hat{c}_i$
- n_1^i = the number of voters (excluding i) in T_1 for whom $c_j < \hat{c}_j$
- n_2^i = the number of voters (excluding i) in T_2 for whom $c_j < \hat{c}_j$
- $P_1^i(\hat{c}_{-i})$ = $\text{Prob}(n_1^i = n_1^i - 1 | \hat{c}_{-i})$
- $P_2^i(\hat{c}_{-i})$ = $\text{Prob}(n_1^i = n_2^i | \hat{c}_{-i})$

Notice that P_1^i and P_2^i correspond to p in equation (1). A Bayesian equilibrium in our model is a set of voter decision rules (critical cost levels), $c^* = (c_1^*, \dots, c_n^*)$ such that

$$c_i^* = P_1^i(c_{-i}^*) \quad \text{for } i \in T_1 \quad (5)$$

$$c_i^* = P_2^i(c_{-i}^*) \quad \text{for } i \in T_2 \quad (6)$$

It can be easily shown that if other voters are using c_{-i}^* , and $i \in T_2$, then voting gives i a higher expected payoff than abstention if and only if $c_i < c_i^*$. To see this, observe that the expected payoff of voting equals $(1 - c_i)$ times the probability that $n_1^i \leq n_1^i$, minus c_i times the probability that $n_1^i > n_1^i$. Hence, for $i \in T_1$ the expected payoff of voting exceeds the expected payoff from not voting if and only if $c_i < c_i^*$. The rationale for equation (6) is virtually identical. That c_i^* must lie between 0 and 1 is obvious, because voters have a dominant strategy to vote if $c_i < 0$ and have a dominant strategy to abstain if $c_i > 1$.

Because all voters in a group face a similar decision problem, it is natural to assume that voters in the same group use the same decision rule in equilibrium. We restrict the analysis to this kind of equilibrium, which we refer to as a *symmetric equilibrium*. This approach simplifies the problem considerably, because a symmetric equilibrium is simply a pair of critical points (c_1^*, c_2^*) such that

³This is one of the more common ways of introducing private information to transform a game of complete information into a game of incomplete information. Everyone knows his own payoff function with certainty, but does not know the other players' payoffs. See, for example, Harsanyi (1967-1968, 1973) and Vickrey (1961).

any voter i in T_1 votes if and only if $c_i < c_1^*$, and any voter i in T_2 votes if and only if $c_i < c_2^*$.

Given any pair of critical points, (c_1, c_2) , we can explicitly write out the formulas for P_1^i and P_2^i , and these are given below. First, let $W_k[N, q] = \binom{N}{k} q^k (1-q)^{N-k}$ represent a binomial probability.

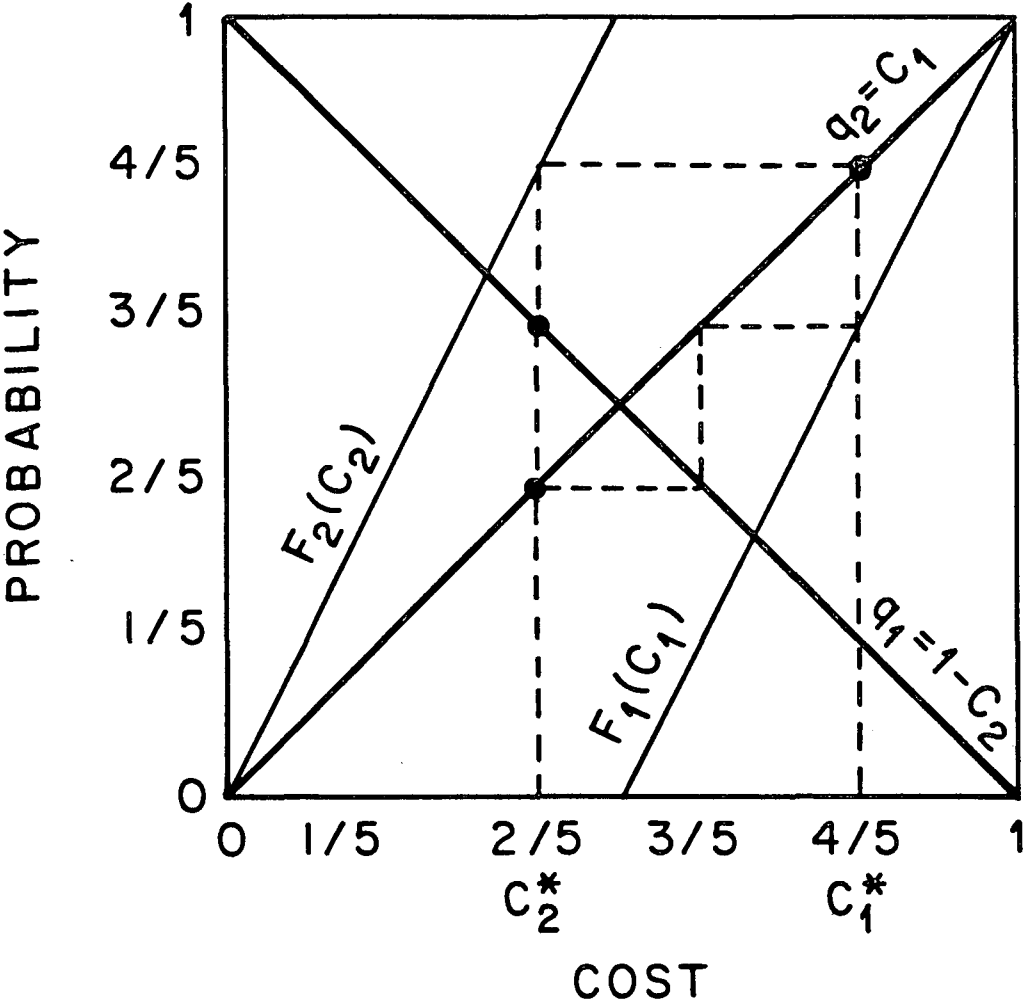
$$P_1^i(c_1, c_2; N_1, N_2, F_1, F_2) = \sum_{k=0}^{\min[N_1-1, N_2-1]} W_k[N_1-1, F_1(c_1)] \cdot W_{k+1}[N_2, F_2(c_2)] \tag{7}$$

$$P_2^i(c_1, c_2; N_1, N_2, F_1, F_2) = \sum_{k=0}^{\min[N_1, N_2-1]} W_k[N_1, F_1(c_1)] \cdot W_k[N_2-1, F_2(c_2)] \tag{8}$$

From equations (5)-(8), the two equilibrium conditions for the critical points are: $c_1^* = P_1^i(c_1^*, c_2^*)$ and $c_2^* = P_2^i(c_1^*, c_2^*)$, or

$$c_1^* = \sum_{k=0}^{\min[N_1-1, N_2-1]} W_k[N_1-1, F_1(c_1^*)] \cdot W_{k+1}[N_2, F_2(c_2^*)] \tag{9}$$

Figure 2. Correspondence between Mixed Strategy and Bayesian Equilibrium When $N_1 = N_2 = 1$



$$c_2^* = \frac{\min[N_1, N_2 - 1]}{\sum_{k=0} W_k[N_1, F_1(c_1^*)]} \cdot W_k[N_2 - 1, F_2(c_2^*)] \quad (10)$$

A symmetric Bayesian equilibrium is represented by any pair of critical costs (c_1^* , c_2^*) that simultaneously solves equations (9) and (10).

In order to illustrate a Bayesian equilibrium and contrast it with an ordinary Nash equilibrium, we consider a simple example where there is only one voter on each team. That is, let $N_1 = N_2 = 1$. The status quo rule breaks ties. In this case, the payoff matrix is given in Table 3.

The mixed strategy Nash equilibrium to this game when c_1 and c_2 are known to both players is calculated readily as: $q_1^* = 1 - c_2$, $q_2^* = c_1$. The solution appears as the heavy cross in Figure 2. So, for example, when $c_1 = 4/5$ and $c_2 = 2/5$, $q_1^* = 3/5$ and $q_2^* = 4/5$.

Now assume that the values of c_1 and c_2 are private information to the respective players. Think of each player as having a cutoff cost level, such that if his own cost is below this level he votes, and otherwise he abstains. In other words, these cutoff levels, designated c_1^* , c_2^* represent the decision rules of the two players.

Recalling equation (1), each voter will want to choose his cutoff level, c_i^* such that $c_i^* = p_i$. For costs above c_i^* , an individual will want to abstain, because the expected reward (R) is negative. For costs below c_i^* , the individual will want to vote. We have: $c_1^* = \text{Prob}\{\text{Voter 1 is decisive given voter 2 uses } c_2^*\}$. Because voter 1 is decisive only in the case where voter 2 votes and hence voter 1 can create a tie and "win" by voting, this expression becomes:

$$c_1^* = \text{Prob}\{\text{voter 2 votes, given voter 2 uses } c_2^*\} = \text{Prob}(c_2 < c_2^*) = F_2(c_2^*)$$

where $F_2(c_2)$ represents the cumulative probability distribution of voter 2's cost. Similarly, for voter 2, we obtain:

$$\begin{aligned} c_2^* &= \text{Prob}(\text{voter 2 is decisive, given voter 1 uses } c_1^*) \\ &= \text{Prob}(\text{voter 1 abstains, given voter 1 uses } c_1^*) \\ &= \text{Prob}(c_1 > c_1^*) = 1 - F_1(c_1^*). \end{aligned}$$

Summarizing, we have two equations in two unknowns:

$$\begin{aligned} c_1^* &= F_2(c_2^*) \\ c_2^* &= 1 - F_1(c_1^*) \end{aligned}$$

At equilibrium, the cutoff levels must be consistent; that is, they must solve the above two simultaneous equations.

In this example, let

$$\begin{aligned} c_1 &\sim U[1/2, 1] \\ c_2 &\sim U[0, 1/2] \end{aligned}$$

That is, both costs have a uniform distribution, but player 1 is the high cost individual. The distribution functions are:

$$\begin{aligned} F_1(c_1) &= 0 & \text{if } c_1 < 1/2 \\ &= 2(c_1 - 1/2) & \text{if } 1/2 \leq c_1 \leq 1 \\ &= 1 & \text{if } 1 < c_1 \\ F_2(c_2) &= 0 & \text{if } c_2 < 0 \\ &= 2c_2 & \text{if } 0 \leq c_2 \leq 1/2 \\ &= 1 & \text{if } 1/2 < c_2. \end{aligned}$$

From the equilibrium condition, we obtain:

$$\begin{aligned} c_1^* &= F_2(c_2^*) \\ &= \begin{cases} 2c_2^* & c_2^* \leq 1/2 \\ 1 & c_2^* > 1/2 \end{cases} \\ c_2^* &= 1 - F_1(c_1^*) \\ &= \begin{cases} 1 & c_1^* < 1/2 \\ 1 - 2(c_1^* - 1/2) & c_1^* \geq 1/2 \end{cases} \end{aligned}$$

As shown in the figure, these equations are solved by $c_1^* = 4/5$, $c_2^* = 2/5$. Finally, note that $F_1(c_1^*) = 3/5$ and $F_2(c_2^*) = 4/5$, so that were these equilibrium cutoff levels the actual costs in the game of complete information, the $F(\cdot)$ values correspond exactly to the mixed strategy probabilities given for these costs.

Another interesting feature of this example is that it illustrates that the equilibrium of the game

Table 3. Payoffs When $N_1 = N_2 = 1$

Type 1	Type 2		
	Vote	Abstain	
Vote	$1 - c_1$	$-c_2$	0
Abstain	0	$1 - c_2$	0

of incomplete information does not correspond to the equilibrium of the "certainty equivalent" game of complete information. This certainty equivalent game with $c_1 = 3/4$ and $c_2 = 1/4$ has equilibrium voting probabilities of $q_1^* = 3/4$, $q_2^* = 3/4$, in contrast to the solution to the incomplete information game which has equilibrium voting probabilities of $F_1(c_1^*) = 3/5$, $F_2(c_2^*) = 4/5$. Conversely, one cannot solve for c_1^* , c_2^* by simply plugging in expected values of the distribution of c_1 and c_2 . Such a procedure would result in the misleading conclusion of $c_1^* = 3/4$, $c_2^* = 1/4$.

Before proceeding, we make two assumptions about the distribution functions, F_1 , F_2 .

ASSUMPTION 1. F_1 , F_2 are continuous on $(-\infty, \infty)$.

ASSUMPTION 2.

(i) $F_1(0) > 0$, $F_2(0) > 0$

(ii) $F_1(1) < 1$, $F_2(1) < 1$

The first assumption rules out mass points in the distribution. The second assumption states that for each group, there is some probability that a voter from that group has negative voting costs (e.g., from citizen duty), and some probability that a voter from that group has sufficiently high costs so that his dominant strategy is to abstain. Assumptions 1 and 2(i) are used only to simplify the presentation. We later discuss relaxing these assumptions. Assumption 2(ii) is not a very strong restriction because $F_1(1)$ and $F_2(1)$ can be arbitrarily close to one. We now state and prove existence of a symmetric equilibrium.

THEOREM 1 (Ledyard, 1981). There exists a symmetric equilibrium (c_A^*, c_B^*) , with $1 \geq c_A^* > 0$, $1 \geq c_B^* > 0$.

Proof. See Appendix A.

Parallels between Equilibria in the Complete and Incomplete Information Games

It is useful to notice some strong parallels between the symmetric equilibrium in the game of incomplete information and totally mixed strategy equilibrium in the game of complete information. Some of these parallels are listed below:

1) If (c_1^*, c_2^*) is a symmetric equilibrium to the game (N_1, N_2, F_1, F_2) , then the complete informa-

tion game in which N_1 players in T_1 have cost c_1^* and the N_2 players in T_2 have cost c_2^* has a TMSE with $q_1^* = F_1(c_1^*)$ and $q_2^* = F_2(c_2^*)$. This is readily verified by observing that equation (2) and the corresponding equation for T_2 can be rewritten:

$$c_i = \frac{\min[N_1-1, N_2]}{\sum_{k=0}^{\min[N_1-1, N_2]} w_k[N_1-1, q_1]} \cdot w_{k+1}[N_2, q_2] \quad \text{for } i \in T_1 \quad (11)$$

$$c_i = \frac{\min[N_1, N_2-1]}{\sum_{k=0}^{\min[N_1, N_2-1]} w_k[N_1, q_1]} \cdot w_{k+1}[N_2-1, q_2] \quad \text{for } i \in T_2. \quad (12)$$

These are also the equilibrium conditions for a TMSE (see Palfrey & Rosenthal, 1983). Substituting c_1^* for c_i , $i \in T_1$ and c_2^* for c_i , $i \in T_2$, $q_1 = F_1(c_1^*)$, $q_2 = F_2(c_2^*)$, equations (11) and (12) coincide with (9) and (10).

2) By a similar argument, given N_1 , N_2 if q_1^* , q_2^* is a TMSE for a complete information game in which voting costs are c_1 for all voters in T_1 and c_2 for all voters in T_2 , then any pair of distribution functions, (F_1, F_2) with the property that $F_1(c_1) = q_1^*$ and $F_2(c_2) = q_2^*$ will support an equilibrium at (c_1, c_2) in the corresponding game of incomplete information.

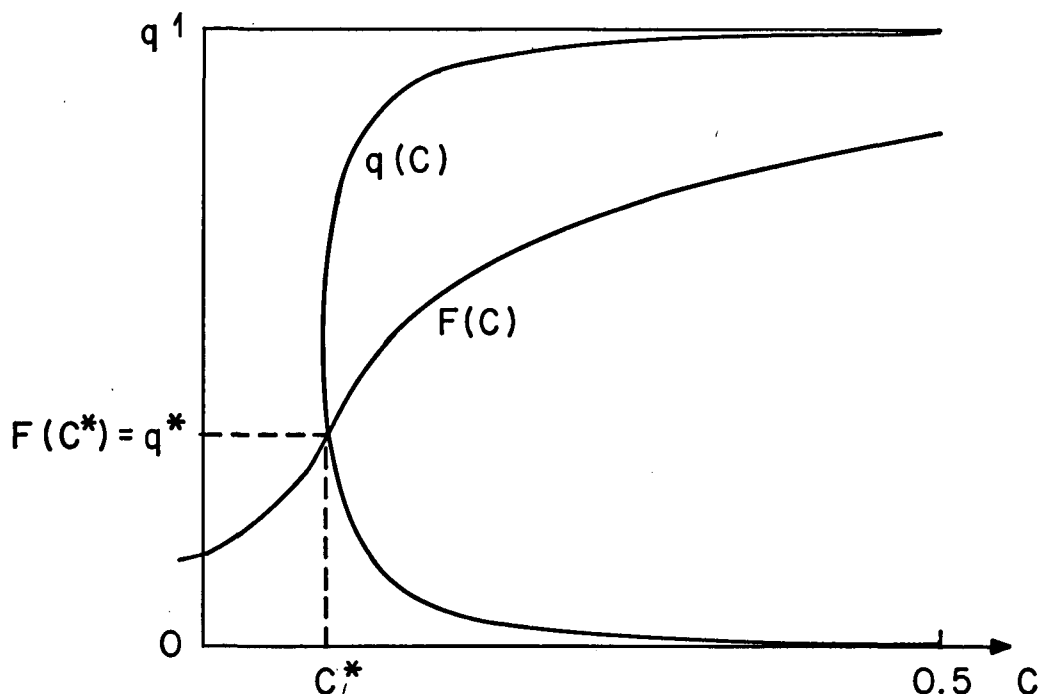
3) The fragile equilibria in the game of complete information require the use of pure strategies by some voters. These pure strategies correspond to decision rules in the game of incomplete information which are completely unresponsive to costs. That such decision rules are never rational follows from Assumption 2, because a voter whose cost is less than zero should vote but should abstain if the cost is greater than one.⁷

The first two parallels described above can be illustrated by a graphical analysis of the coin-toss example above. Consider the incomplete information game analogous to the complete information game in which $N_1 = N_2$ and all voters have the same cost. In this corresponding game, we let the distribution of voting costs be the same for members of both groups. That is, $F_1 = F_2$. Because $N_1 = N_2$, it follows that in equilibrium $c_1^* = c_2^* = c$ and $F_1(c_1^*) = F_2(c_2^*)$. That is, all voters use the same critical cost level to decide whether or not to vote, which corresponds to the equilibrium out-

⁷Under the coin-toss rule, abstention dominates for $c_i > 1/2$.

⁷Note that the existence of "fragile" equilibria in the game of complete information is not dependent on the assumption of identical costs. For example, if some voters have low cost $c_l \in (0, 1)$ and others have high cost $c_h > 1$, the c_l voters play the same game as they would play were there no c_h voters.

Figure 3. Relationship between Nash Equilibrium and Bayesian Equilibrium



come of one particular game of complete information, where all voters have cost c^* and vote with probability $q^* = F(c^*)$. This is illustrated graphically by superimposing the distribution function of voting costs onto the complete information game equilibrium correspondence of Figure 1. (See Figure 3.) The intersection between $F(c)$ and $q^*(c)$ gives c^* , the symmetric Bayesian equilibrium of the game of incomplete information.

Bayesian Equilibrium in Large Electorates

Downs's longstanding conjecture that in a large electorate voters with positive voting costs will abstain is considered in this section. Before explicitly formalizing this hypothesis about the effect of electorate size, the intuition of how the Bayesian equilibrium changes as N_1 and N_2 change is illustrated using the same example in which $N_1 = N_2$ and $F_1(\cdot) = F_2(\cdot)$.

If N_1 and N_2 are increased simultaneously, then it can be shown that the $q^*(c)$ curve tends toward the q -axis in a particular way, as illustrated in Figure 4. One can see from the graph that as N increases, the point of intersection of the distribution function and the $q^*(c)$ curve, which deter-

mines the Bayesian equilibrium, c^* , converges to $(0, F(0))$. This is consistent with the Downs-Riker-Ordeshook-Tullock view that the only reason a rational voter would vote in a large election is if his net voting cost is negative.

We now prove formally that the limiting property illustrated above in the example game is true in general. First we need to introduce the following notation. Fix F_1, F_2 . Consider the sequence of values of $N_1 = 1, 2, 3, \dots$ and any sequence of positive integer values of $N_2 = N_2^1, N_2^2, \dots$. For any j let Γ_j be the game with $F_1, F_2, N_1 = j, N_2 = N_2^j$. Let $\{(c_{1j}^*, c_{2j}^*)\}_{j=1}^\infty$ denote any sequence of equilibria to $\{\Gamma_j\}_{j=1}^\infty$.

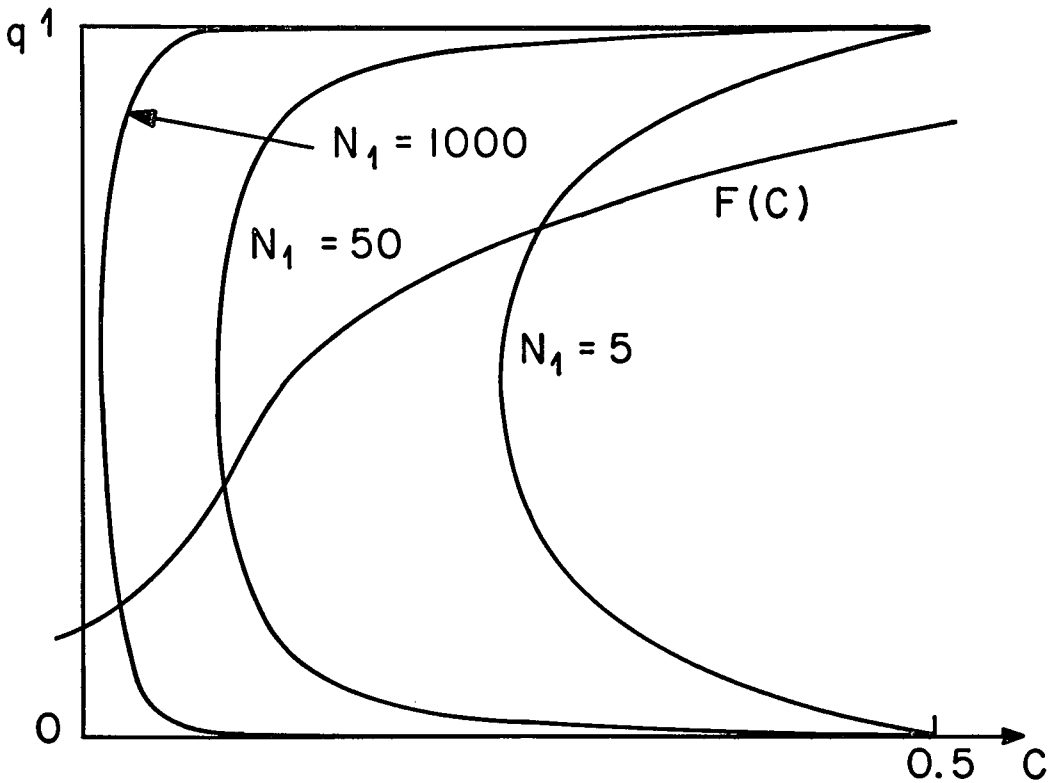
THEOREM 2. $\lim_{j \rightarrow \infty} c_{1j}^* = 0$.

Proof. See Appendix A.

Notice that these limiting results do not depend on the sequence $\{N_2^j\}_{j=1}^\infty$. A similar result for c_{2j}^* can be obtained if we let $\{N_1^j\}_{j=1}^\infty$ be arbitrary and let $N_2^j = j, j = 1, 2, \dots$. Hence, for either group the cut point approaches 0 as that group gets large, regardless of the size of the other group.

Assumptions 1 and 2 were made in order to simplify the proof. However, this convergence result can be proved more generally. Because such a proof is straightforward but overly tedious, the

Figure 4. Equilibrium as a Function of Electorate Size



role of these two technical assumptions is simply illustrated in Appendix B using the coin-toss example.

Therefore in very large electorates where N_1 and N_2 are both large, both critical cost levels are approximately zero. In other words, voters will not vote in large elections if the net cost is positive. Thus, at least according to this strictly game theoretic framework, outcomes are determined by the relative sizes of the negative cost subgroups within T_1 and T_2 .

Preference Uncertainty

The kind of preference uncertainty we deal with in this section is uncertainty about N_1 . We assume that the total number of voters, N , is known, but the number of voters supporting alternative 1 is not known. Each voter knows which alternative he supports, but knows only the probability distribution over the proportion of the other $N-1$ voters who favor alternative 1. We denote the probability that j of those $N-1$ voters prefer alternative 1 by $P_N(j)$, where $P_N(j) \geq 0$ and

$$\sum_{j=0}^{N-1} P_N(j) = 1.$$

We retain the rest of the structure developed in earlier sections of this article. Again, an equilibrium to the game with N voters is a pair of critical cost levels, c_{1N}^* , c_{2N}^* such that the expected payoff from voting for a group 1 (group 2) voter with cost c_1^* (c_2^*) equals the expected payoff from abstention. A symmetric equilibrium is characterized by the following two equations:

$$c_{1N}^* = \sum_{j=0}^{N-1} \sum_{k=0}^{\min(j, n-j-2)} w_k [j, F_1(c_{1N}^*)] \cdot w_{k+1} [N-j-1, F_2(c_{2N}^*)] \cdot P_N(j) \quad (13)$$

$$c_{2N}^* = \sum_{j=0}^{N-1} \sum_{k=0}^{\min(j, N-j-1)} w_k [N-j-1, F_1(c_{1N}^*)] \cdot w_k [j, F_2(c_{2N}^*)] \cdot P_N(N-j-1) \quad (14)$$

In order to take limits on (c_{1N}^*, c_{2N}^*) , we must specify how $P_N(j)$ changes as N gets large. Because we are interested in *large* electorates, we make the following assumption about the limiting properties of the sequence $\{P_N(\cdot)\}_{N=2}^\infty$:

ASSUMPTION 3. For all T , $\lim_{N \rightarrow \infty} \sum_{j=T}^{N-T} P_N(j) = 1$.

This assumption guarantees that in the limit, N_1 and N_2 will be infinite with probability 1, which is precisely the type of sequence we wish to consider. Given some F_1 , F_2 and an infinite sequence $\{P_N(\cdot)\}_{N=1}^\infty$, let $\{(c_{1N}^*, c_{2N}^*)\}_{N=2}^\infty$ be any sequence satisfying (13) and (14) for all N .

THEOREM 3. If F_1 , F_2 and $\{P_N(\cdot)\}_{N=1}^\infty$ satisfy Assumptions 1-3, then

$$\lim_{N \rightarrow \infty} c_{1N}^* = 0$$

$$\lim_{N \rightarrow \infty} c_{2N}^* = 0$$

Proof. See Appendix A.

Conclusions and Normative Implications

In this article we have shown that in the presence of a relatively small degree of strategic uncertainty, at a Bayesian equilibrium, voters with positive net voting costs will abstain. The outcome will essentially be determined by voters who, for unspecified reasons, find it at least as costly to abstain as to vote, regardless of the likelihood that their vote will affect the outcome of the election.

The fact that voters with positive voting costs abstain implies that the outcome of an election need not reflect the underlying distribution of preferences in the population. If the distribution of cost-benefit ratios for the portion of the population that favors one alternative is identical to the distribution for those who favor the other alternative, the majority side can be expected to win.* But if the low and negative cost individuals are concentrated in the minority, the minority can win.

This hypothesis suggests a useful direction for further research. An important aspect of competition between candidates must involve the mobili-

zation of support groups. Thus, much work in formal theory which focuses almost exclusively on ideological or spatial competition is missing a key ingredient of electoral competition. In order to win a vote, a candidate must not only convince the voter that he or she is the preferred candidate but must also get these voters to the polls. Exactly how to combine these two problems in a coherent theoretical model is an open and difficult question. Campaign organization would obviously be a critical aspect of such a model. Parties, as stable, institutionalized organizations, might emerge endogenously as efficient means for both generating support for candidates and getting out the vote. Also important, the distribution of voting costs, exogenously given in our model, would be endogenously determined in a more general theory of electoral competition.

This more general theory would emphasize candidate strategies designed to affect the cost of voting in a way that increases the turnout of likely supporters. These strategies might alter the cost of voting directly, as in providing transportation to the polls, or indirectly, as in providing cash or services contingent on monitoring turnout. In some situations, organized interests may use monitoring to increase the cost of voting. For example, in 1974, the French Communist Party advocated abstention in the presidential runoff between the Gaullist Georges Pompidou and the Centrist Alain Poher. Record abstention resulted, perhaps influenced by the ability of party poll watchers to monitor voting. Less subtle tactics have been used to disenfranchise entire blocs of voters in the United States (Key, 1949; Kousser, 1974).

A key feature of voting institutions in modern democracies is that turnout can in fact be monitored while choice cannot. Positive theory is needed to explain this regularity. We note that if sanctions cannot be imposed for nonvoting and rewards offered for voting, the net cost of voting would increase and turnout would fall.

We also note that the cost of voting appears to be small. Consider the following observations. 1) Fines for not voting are often modest in democracies with compulsory registration. 2) Wolfinger and Rosenstone (1980, p. 129) estimate positive effects on turnout for some simple changes in registration procedures, such as maintaining evening and Saturday office hours. 3) Perform your own Gedanken experiment on the likely effect of turnout in U.S. presidential elections if each voter were handed \$20 in cash after voting. If costs are low, turnout is likely to be very responsive to mobilization efforts by candidates and parties.

In addition to providing some suggestions for further work in positive theory, our work also highlights an important tradeoff from a norma-

*Although this may seem a rather obvious conclusion to most readers, it is worth pointing out that with complete information, this is not necessarily true. At a Nash equilibrium with complete information, if all voters have the same voting costs, the minority candidate has a substantial chance of winning even when the majority vastly outnumbers the minority. For details, see Palfrey and Rosenthal (1983).

tive viewpoint. As resources are spent on mobilizing voters, increased participation is likely to increase the chances that the majority side will win, as it must when there is total participation. Most proponents of majority rule would thus find increased participation socially desirable. But the expenditure of resources to increase participation is a cost to society. Thus, the theory of the efficiency of elections (see Ledyard, 1984; Palfrey & Rosenthal, 1983) remains an open question. A model that addresses mobilization issues directly is needed if we are to make headway answering this normative question as well as positive questions about the nature of electoral competition.

Appendix A. Proofs of Theorems

Proof of Theorem 1. We first prove that there exists an equilibrium with $(c_A^*, c_B^*) \in [0,1] \times [0,1]$ and then show that both critical points must be strictly positive. Consider the mapping from $[0,1] \times [0,1]$ into itself defined by

$$y_1 = P_1(c_1, c_2)$$

$$y_2 = P_2(c_1, c_2)$$

with P_1 and P_2 as defined in equations (7) and (8). Because P_1 and P_2 are continuous functions and $[0,1] \times [0,1]$ is compact and convex, by Brouwer's fixed point theorem there exists a fixed point (y_1^*, y_2^*) such that

$$y_1^* = P_1(y_1^*, y_2^*)$$

$$y_2^* = P_2(y_1^*, y_2^*)$$

Let $(c_1^*, c_2^*) = (y_1^*, y_2^*)$. Then (c_1^*, c_2^*) is a symmetric equilibrium in $[0,1] \times [0,1]$. To show the critical points are strictly positive, suppose $c_1^* = 0$. Then it must be the case that $P_1(c_1^*, c_2^*) = 0$. But this is impossible because $F_1(c_1^*)$, $(1 - F_1(c_1^*))$, $F_2(c_2^*)$, $(1 - F_2(c_2^*))$ are all strictly positive by Assumption 2.

Q.E.D.

The proof of Theorem 2 uses Dini's theorem on uniform convergence, stated below.

DINI'S THEOREM. Suppose that (f_n) is a sequence of continuous functions on a compact set, K , which converges monotonically to a continuous function f on K pointwise. Then (f_n) converges to f uniformly on K .

Proof of Theorem 2. We construct an upper bound sequence $(\bar{c}_j)_{j=1}^\infty$ such that $c_{1j}^* < \bar{c}_j$, $j = 1, 2, \dots$. We

then establish that $\lim_{j \rightarrow \infty} \bar{c}_j = 0$. It follows immediately that $\lim_{j \rightarrow \infty} c_{1j}^* = 0$.

By equation (9), c_{1j}^* must satisfy

$$c_{1j}^* = \frac{\min[j-1, N_2^j-1]}{\sum_{k=0}^{\min[j-1, N_2^j-1]} W_k[j-1, F_1(c_{1j}^*)]} \cdot W_{k+1}[N_2^j, F_2(c_{2j}^*)]$$

Since $\sum_{k=0}^{N_2^j-1} W_{k+1}[N_2^j, F_2(c_{2j}^*)] \leq 1$,

$$c_{1j}^* \leq Q_j^* = \sum_{k=0}^{N_2^j-1} W_{k+1}[N_2^j, F_2(c_{2j}^*)] \leq Q_j^*$$

where $Q_j^* = \max_{c_1, k \leq j-1} W_k[j-1, F_1(c_1)]$.

Define $Q_j^*(c_1) = \max_{k \leq j-1} W_k[j-1, F_1(c_1)]$.

We next establish that $Q_j^*(c_1) \geq Q_{j+1}^*(c_1)$ for all $c_1 \in [0,1]$. To see this, choose c_1 arbitrarily between $[0,1]$. Then $Q_j^*(c_1) = W_{k^*}[j-1, F_1(c_1)]$ where k^* is the unique integer between $jF_1(c_1)-1$ and $jF_1(c_1)$. (See Feller, 1957, p. 140.) Hence $Q_{j+1}^*(c_1)$ equals either $W_{k^*}[j, F_1(c_1)]$ or $W_{k^*+1}[j, F_1(c_1)]$. In the first case,

$$\begin{aligned} \frac{Q_j^*(c_1)}{Q_{j+1}^*(c_1)} &= \frac{(j-1)^{k^*} [F_1(c_1)]^{k^*} [1-F_1(c_1)]^{j-1-k^*}}{(k^*)^j [F_1(c_1)]^{k^*+1} [1-F_1(c_1)]^{j-k^*}} \\ &= \frac{j-k^*}{j(1-F_1(c_1))} = \frac{j-k^*}{j-jF_1(c_1)} > 1 \end{aligned}$$

because $k^* \leq jF_1(c_1)$.

In the second case,

$$\begin{aligned} \frac{Q_j^*(c_1)}{Q_{j+1}^*(c_1)} &= \frac{(j-1)^{k^*} [F_1(c_1)]^{k^*} [1-F_1(c_1)]^{j-1-k^*}}{(k^*+1)^j [F_1(c_1)]^{k^*+1} [1-F_1(c_1)]^{j-1-k^*}} \\ &= \frac{k^*+1}{jF_1(c_1)} > 1 \end{aligned}$$

because $k^* > jF_1(c_1) - 1$.

Hence, we have that $Q_j^*(c_1)$ is monotonic in j . Moreover, by Assumption 2, $Q_j^*(c)$ converges pointwise to 0 on $[0,1]$. Therefore, by Dini's theorem, $Q_j^*(c_1)$ converges to 0 uniformly on $[0,1]$. Then $Q_j^* =$

$\max_{c_1 \in [0,1]} Q_j^*(c_1)$ converges to 0. (See Rudin, 1976, Theorem 7.9, p. 148.) Now just let $\bar{c}_j = Q_j^*$, $j = 1, 2, \dots$, and we have a sequence with the desired properties. Hence $\lim_{j \rightarrow \infty} c_{1j}^* = 0$.

Q.E.D.

Proof of Theorem 3. For each integer $t \geq 0$, define N_t as the least integer such that for all $N \geq N_t$, $\sum_{j=0}^t P_N(j) < \frac{1}{t+1}$. By assumption 3, N_t is well defined for every $t \geq 0$. Next, for each integer N , define $t(N)$ as the largest integer such that $N_{t(N)} \leq N$. Because by construction, N_t is an unbounded, monotonically increasing sequence, $t(N)$ is also well defined and generates an unbounded monotonically increasing sequence.

From these definitions, it follows immediately that

$$c_{1N}^* < \frac{1}{1+t(N)} + \sum_{j=t(N)}^{N-1} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)] P_N(j) < \frac{1}{1+t(N)}$$

$$+ \max_{t(N) \leq j \leq N} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)].$$

Taking limits, we have

$$\lim_{N \rightarrow \infty} c_{1N}^* \leq \lim_{N \rightarrow \infty} \frac{1}{1+t(N)} + \lim_{N \rightarrow \infty} \max_{t(N) \leq j \leq N} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)].$$

By construction,

$$\lim_{N \rightarrow \infty} \frac{1}{1+t(N)} = 0.$$

By Theorem 2,

$$\lim_{N \rightarrow \infty} \max_{t(N) \leq j \leq N} \sum_{k=0}^{\min(j, N-j-2)} W_k[j, F_1(c_{1N}^*)] \cdot W_{k+1}[N-j-1, F_2(c_{2N}^*)] = 0.$$

Hence, $\lim_{N \rightarrow \infty} c_{1N}^* = 0$.

A similar argument is used to show

$$\lim_{N \rightarrow \infty} c_{2N}^* = 0.$$

Q.E.D.

Appendix B. The Role of Assumptions

The coin-toss example is used here to illustrate the role of Assumptions 1 and 2 in the model.

ASSUMPTION 1. If the distribution function is discontinuous, then the simple existence proof doesn't work, because the fixed point mapping will not be continuous. Such a situation in which a pure strategy symmetric equilibrium as we have defined it does not exist is illustrated in Figure 5.

The source of the discontinuity is a mass point in the distribution of costs at $\bar{c}$. If all voters with costs less than or equal to $\bar{c}$ vote, then $F(\bar{c}^*) > q^*$, whereas if only voters with costs strictly less than $\bar{c}$ vote, then $F(\bar{c}^*) < q^*$. However, a symmetric mixed strategy equilibrium does exist in this example. If the strategy described is amended so that voters with costs exactly equal to $\bar{c}$ vote with probability

$$\frac{q^* - \underline{F}(\bar{c})}{\bar{F}(\bar{c}) - \underline{F}(\bar{c})}$$

then it is as if $F(\bar{c}^*) = q^*$ and the equilibrium conditions 2-4 are satisfied. Thus, from this example it is clear that a mixed strategy Bayesian equilibrium will exist in the absence of Assumption 1, and these equilibria will share the convergence property of Theorem 2.

ASSUMPTION 2(i). We assume that there is some chance that some voters have a negative net voting cost (citizen duty). In contrast, Ledyard (1981, 1984) assumed that there were no such voters. When this happens, $F(0) = 0$, and the graph looks like Figure 6, where $\underline{c}$ is the lowest possible voting cost.

In this case $c_n^* \rightarrow \underline{c} > 0$, and turnout is infinitesimal (0%) in large electorates. Although this is not a citizen-duty story, it is similar in the sense that for any $c > 0$ in the support of F , there exists $\bar{N}$ sufficiently large such that $c_N^* < c$ for all $N > \bar{N}$.

ASSUMPTION 2(ii). Assumption 2(ii) is needed to guarantee that in large electorates essentially the only voters are citizen-duty voters (i.e., $c_i \leq 0$). Suppose in the example $F(c) = 1$ for $c \geq 3/8$. This situation is illustrated in Figure 7.

Figure 5. Nonexistence of Pure Strategy Symmetric Equilibrium Due to Discontinuity of the Distribution Function

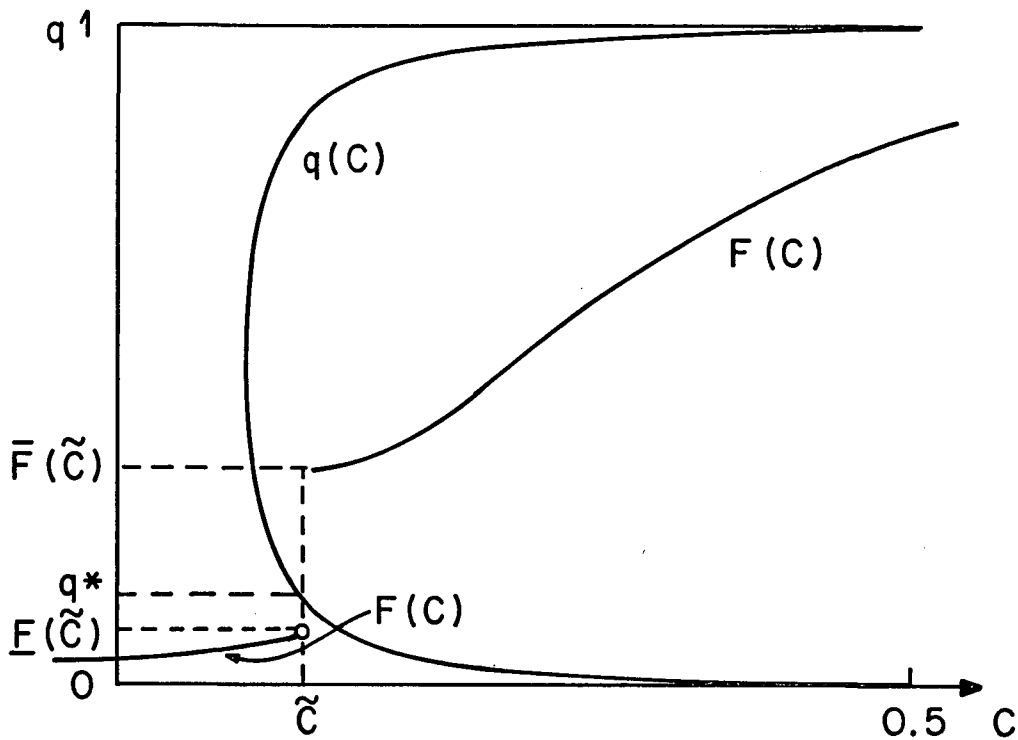


Figure 6. Convergence if $F(0) = 0$

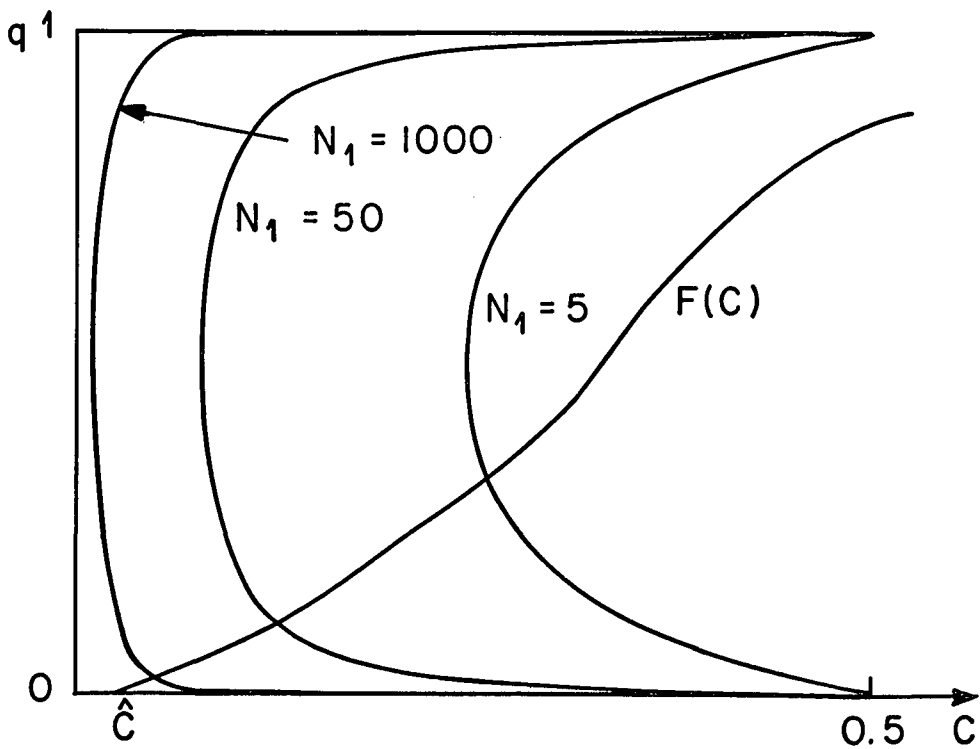
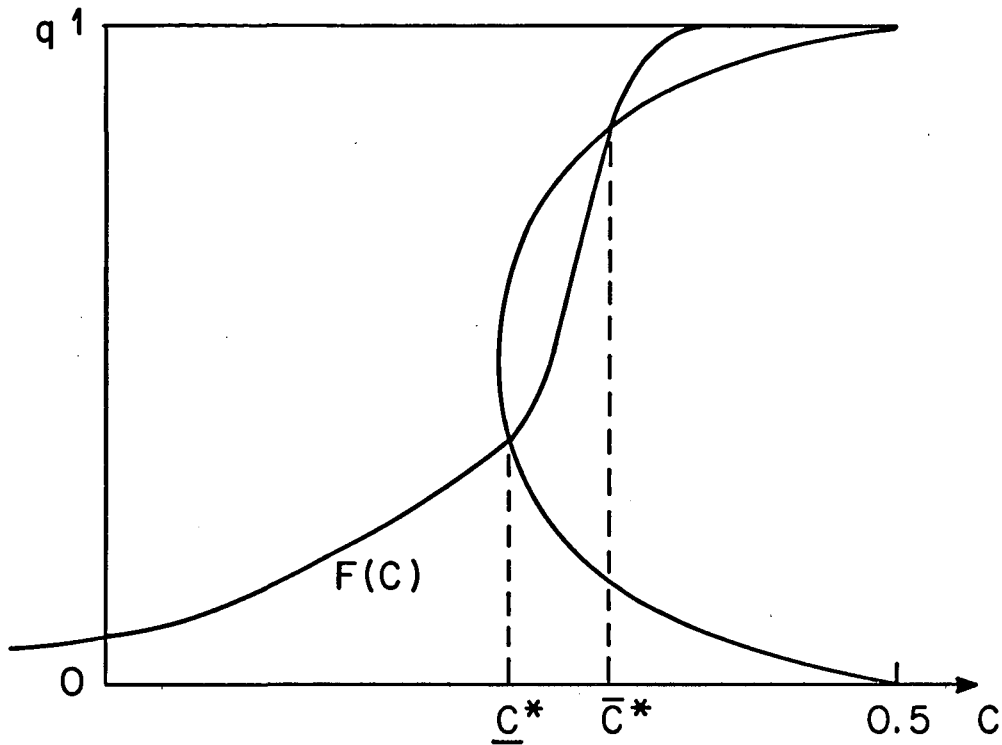


Figure 7. Equilibrium if $F(3/8) = 1$ 

In such a case there may be more than one equilibrium. One at $\underline{c}^*$ (which in the limit corresponds to the citizen duty equilibrium), and one at $\bar{c}^*$, which is a high turnout equilibrium. There is also a third "degenerate" equilibrium in which everyone votes, regardless of voting costs. This corresponds to the pure strategy equilibrium identified for this special case in Palfrey and Rosenthal (1983). What is happening here is that there is not enough uncertainty if $F(c) = 1$ for some $c < 1/2$, because the probability that a voter will have a dominant strategy of not voting is zero.

The purpose of this extended example has been to illustrate the role of Assumptions 1 and 2. Assumption 1 is technically convenient and innocuous. As for Assumption 2, it can be shown that in more general cases (i.e., $F_1 \neq F_2$, $N_1 \neq N_2$), the same variations in the equilibria as in the example will emerge when Assumption 2 is relaxed. Assumption 2(ii) is used primarily because we feel it is a realistic assumption that some voters find it too costly to vote, regardless of the probability that they would be decisive. Assumption 2(i) is technically convenient and does not change the qualitative nature of the results at all.

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The Political Economy of Group Membership

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Despite its normative importance, the question of why people join interest groups remains open. It has certainly provoked a wealth of theoretical attention. Regrettably, however, it has inspired only a handful of empirical tests. The introduction of this article places the empirical debate into its normative context. The first section develops a rational model of individual evaluations of group membership, focusing upon the effect of changing personal circumstances—preferences, needs, resources, insecurity, and information—on the calculus. In particular, the theory predicts responsiveness to political or collective benefits in threatening times. Analyses of aggregate changes over time in membership in the Farm Bureau, the League of Women Voters, and the Home Builders, reported in the second section, bear the model out. Finally, the conclusion takes on the complementary question of group supply, sketching a theory of group mobilization that emphasizes subsidization.

Observers of American democracy have greeted political interest groups with praise and with condemnation, but seldom with both. According to some commentators, associations are the upholders of the interests of intense minorities; according to others, they are the selfish saboteurs of majority rule.

These normative conclusions, however, depend vitally on an empirical premise about why people join interest groups. If, as Truman (1971, pp. 26-43) hypothesized, people create and join organizations in response to "disturbances" in the social environment, then interest groups represent legitimate grievances that should be heeded in a democratic political system. But suppose, as Olson (1965) argued, that people who share interests with a large group join less readily than people who share interests with a small one, and suppose that joiners of large groups act in the collective interest only as an unintended consequence of their seeking benefits unrelated to the association's political purpose. Then interest

groups, if they have any influence at all, bias public policy away from majority preferences.

Here I argue that political mobilization of large groups does indeed reflect political concerns, but only under certain conditions. I develop a model of interest group membership in which group incentives interact with individual circumstances and test the model's propositions against membership data from three prominent lobbies. My conclusions are at once encouraging and sobering. Political benefits do matter, especially when groups are threatened, but they do not always matter.

A Context-Sensitive Model of Interest Group Membership

Olson's by-product theory focuses on two classes of benefits derived from group membership: collective and selective (Olson, 1965, Chap. 6). Collective or political benefits (I use the terms interchangeably) are received jointly by both members and nonmember constituents of the group, and for that very reason are insufficient inducements toward activity. Selective benefits, in contrast, are essential for cooperation because they can be provided to dues payers alone. In concept, the class of selective inducements is varied, although Olson by that term understands goods and services that can be expressed monetarily. Others cast the term more broadly, adding intangible inducements of two kinds: solidary benefits—"rewards created by the act of associating" like fun and friendship—and expressive benefits—"rewards that derive from a sense of satisfaction at having contributed to the

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attainment of a worthwhile cause" (Wilson, 1973, p. 34).

The context of the incentive model remains unspecified, however, and explicitly or implicitly, interest group theorists assume three things about it: the existence of associations offering political benefits, the extent and nature of the information available to individuals, and the configuration of individual preferences and resources. The first, of course, is simply an assumption that interest groups exist, that they supply the incentives outlined above at some cost, and that some of the benefits supplied are political. I, too, follow convention and assume that interest groups exist, although I return to the question of supply in the conclusion.

The content of the last two assumptions, my primary concern here, is usually stated as fixed and immutable, if indeed it is stated at all. But no single set of assumptions about information, preferences, and resources is universally applicable; rather, the set of appropriate assumptions changes as circumstances change. People in different contexts have different information about alternatives. Depending upon the circumstances, people will be differentially uninformed and misinformed about the actual benefits, costs, and risks of collective action.

More important, people in different contexts have different preferences and resources and hence different subjective weightings of the benefits and costs of group participation. When resources like income and time are ample, people can more easily bear costs; when people have particular needs and preferences, they are more attracted by certain benefits; and when people take different attitudes toward risk, they are more or less willing to engage in actions whose success is uncertain and contingent on the actions of others. In short, information, preferences, and resources that arise in particular situations interact with the actual incentives that organizations offer, forming the subjective assessments of benefits and costs that enter into personal calculations. In equation form, this conceptual model of the individual calculus is:

$$M = E(B_g^s) + \sum B_s^s - \sum C^s \\ = (B_g^a I_g T_g)(P I_p A_p) + \sum (B_s^a I_s T_s) \\ - \sum (C^a I_c R_c) \quad (1)$$

where

M = membership (if $M > 0$, the individual joins; if $M \leq 0$, the individual does not)

B = benefits

P = probability that the collective good will not be provided if the individual does not join

C = costs

I_g, I_p, I_s, I_c = information about collective benefits, probabilities, selective benefits, and costs

T = preferences and needs

R = resources

A = risk attitudes

subscript g = collective or political benefits

subscript s = selective benefits (services, and solitary and expressive benefits)

superscript s = subjective levels

superscript a = actual levels

An individual joins an interest group if the subjective benefits (B^s) he derives from membership exceed the subjective costs (C^s) he incurs. His subjective assessments depend, however, upon the information (I) that he has about actual benefits (B^a), costs (C^a), and probabilities (P) (cf. Moe, 1980). In addition, the assessments depend upon environmental and dispositional factors that impinge upon specific elements of the calculus: resources (R) on costs, preferences (T) on benefits, and risk attitudes (A) on probabilities. As information, resources, preferences, and risk attitudes change, so too does the attractiveness of group affiliation.

To see how they change, consider in turn each of the three major terms in equation (1).

Expected Political Benefits:

$$E(B_g^s) = (B_g^a I_g T_g)(P I_p A_p)$$

At base, collective action is a strategic problem (Hardin, 1982). The benefits of cooperation indisputably are large. Even a small construction firm with a \$100,000 payroll (approximately 10 employees), for example, will save \$1000 a year if the unemployment tax is reduced by a single percentage point. But if the political good is won, group constituents receive the windfall regardless of whether or not they cooperated. Given the choice, then, a prospective member would rather remain prospective, reaping any benefits while avoiding all costs.

The strategic aspect, then, is in avoiding costs while being sure that the benefits keep coming. The individual has some idea of what the present and potential benefits are worth to him (B_g^s). He also has some notion of the likelihood that those benefits will *not* be provided, now or in the future, if he does not cooperate (P).¹ In large

¹This conditional probability corresponds to Hardin's (1982, chaps. 9-11) notion of collective action as an

groups and in normal times, the product of the two, the expected political benefits $[E(B_q^s)]$, will for many people be smaller than costs, although not perhaps as small as some have imagined.

Threatening times, be they political, economic, or social, alter three of the premises that underlie the conclusion that political benefits lack motivational force in large groups. First, different kinds of information are available and different kinds of information are salient in times of threat. Groups are always fending off threats; that is, the benefits are always there, but people do not notice them until they are brought to their attention. Threats bring defensive benefits to people's attention. As the adage goes, "You don't know what it's worth until you need it."

Second, potential losses weigh more heavily in people's minds than potential gains (Kahneman & Tversky, 1979). In other words, the utility of a given amount lost exceeds the utility of the same amount gained.

Finally, threats change people's risk attitudes. Faced at most times with potential political gains or at worst small losses, people are risk averse. Given a choice between a certain outcome and a probabilistic outcome of slightly greater expected value, they will choose the sure thing. Faced with large losses, however, people are risk seeking. They will gamble on a probabilistic loss of greater expected value rather than take a sure loss (Kahneman & Tversky, 1979). Realistically, a single individual's contribution to an interest group will make no more difference to its ability to ward off a loss than its ability to secure a gain, but people are more willing to take a chance that it might avert a loss.

In sum, people are more easily mobilized in response to threats than in response to prospects. Threats increase awareness (I_g) of the collective benefits of group membership. Political benefits that avoid losses are weighed more heavily than political benefits that promise gains (i.e., T_g is higher). And instead of discounting collective benefits for risk, threatened people place a premium upon them (i.e., A_p changes from less than one to greater than one). This is the insight that prompted Truman's (1971, pp. 26-43) emphasis on the motivational force of socioeconomic disturbances, and this is the insight that inclines interest group leaders to emphasize threats over prospects in their appeals. The environmental lobbies, for example, stress the environmental deter-

ioration that will occur if the person solicited does not act, rather than the environmental improvement that will come about if he does (Mitchell, 1979). After James Watt's resignation, these same groups scrambled to find a new *bête noire* as convincing as Reagan's Secretary of the Interior.

The motivational force of political benefits also varies, of course, with other elements of context. First, actual political benefits (B_q^a) change as the political agenda changes. Promoting or staving off regulation of business, for instance, is a political benefit only if policymakers can conceive of business regulation. Second, the probability that collective goods will not be provided if an individual does not cooperate (P) decreases, roughly, with group size. If the political context (for instance, a decentralized political system) makes possible provision of collective goods by local chapters, then a national federation can provide still other benefits on a more inclusive scale (cf. Hardin, 1982, pp. 195-197).

Political benefits, in short, will have different motivational value in different milieux. Like any other good, they are more or less in demand according to people's needs at the moment.

Selective Benefits: $\Sigma (B_s^s) = \Sigma (B_s^a I_s T_s)$

As with collective benefits, demand is more stable for some selective incentives than for others. Some services, such as insurance, are necessities. As a Farm Bureau officer pointed out, "You don't cancel your insurance when things get bad." On the other hand, solidary and expressive benefits by and large are luxury goods. This distinction is implicit in the frequently made assertion that associations offering only intangible incentives are less stable than those offering tangible benefits (Salisbury, 1969; Wilson, 1973), but it need not be unalterably true. For many people, for instance, religious rewards are more important than discount travel tours, and organizations offering the former intangible benefit are apt to be more enduring than those offering only the latter tangible good.

Still, the generalization that services are often necessities whereas expressive and solidary incentives are luxuries allows three predictions. First, demand for intangible incentives will be highly income elastic—that is, small fluctuations in income will produce large shifts in quantity demanded—whereas demand for tangible incentives will be income inelastic. Consequently, income changes that scarcely disturb interest groups offering tangible benefits may be catastrophic for groups relying on intangible inducements. Second, demand for intangibles will be quite price elastic, that is, sensitive to the level of dues. Price elasti-

iterated game in which anticipated reprisals of non-cooperation for noncooperation provide an incentive for cooperation that would not exist in single play games.

city will be higher still if close substitutes exist.² Because substitutable expressive and solidary benefits are so widely available—and not only in organizations—interest groups relying upon intangibles are all the more vulnerable to income and price fluctuations (cf. Knoke & Wright-Isak, 1982, p. 243).

Finally, demand for intangible selective goods will be extremely sensitive to changes in fashion. The Sierra Club, for example, has existed since 1892, yet only recently has it attracted a large following. Clearly, part of the reason is the heightened salience of conservation in the wake of Earth Day in 1970. In some circles Sierra Club membership is a status good, a symbol of the member's environmental consciousness. Sierra, like the other public interest lobbies, traffics in status. In exchange for dues no more costly than a subscription to the *New Yorker*, Sierra Club members receive a handsome magazine for their coffee tables and a distinctive decal for their car windows.

To sum up, demand for selective material incentives depends less upon context than demand for intangible selective goods. Stability of demand for incentives translates into membership stability, if members are equally able to bear costs.

$$\text{Costs: } \Sigma C^s = \Sigma (C^a I_c R_c)$$

Material costs, such as dues and demands on time, often are not the only price of membership. Costs might also be solidary or expressive. Many people, for example, are put off by Ralph Nader. For them, associating with Nader would be a solidary cost of joining Public Citizen. Likewise, imagine the expressive costs faced by a liberal outdoor enthusiast who joins the National Rifle Association to obtain discounts at hunting lodges (cf. Moe, 1980, pp. 608-609). In the two examples, the tastes that interact with actual costs are clear: expressive costs of NRA membership are higher for ideological liberals than for conservatives.

In general, however, for the same reasons outlined in the preceding section, the most important costs are material. People join interest groups by and large because they can afford it; studies of

interest groups virtually always find income effects (e.g., Salisbury, 1969; Tontz, 1964). Yet high income hardly implies infinite resources. When consumers choose to join interest groups, they forgo other products they might have purchased, other avenues of political participation (e.g., contributing to parties or candidates), or other consumption goods. Hence, subjective costs will be altered by the existence of substitutes (cf. Hirschman, 1971, chap. 2). Fulltime employment and membership in the League of Women Voters, for example, require an identical resource (time) and offer similar benefits (social interaction and intellectual engagement). Thus, when career opportunities for women increase, as they did during the seventies, the opportunity costs of membership in a participatory group like the League also increase.

Additivity

In some cases, the same contextual factor interacts with both benefits and costs, but because benefits and costs are additive, the ultimate effect on participation depends upon whether the factor affects benefits or costs more strongly. Income, for example, clearly is a resource that enables one to bear material costs. Some people, however, use income as a psychological indicator of personal economic well-being, affecting how they weight political benefits. The total effect of a change in income, then, depends upon which interaction is stronger.

The model's additivity also underscores the importance of quantity and quality of information. People obviously lack the time and capacity to perform all of the calculations seemingly required by equation (1). In everyday life, though, the decisional elements that people consider are the ones that are available, those that most easily come to mind (Taylor, 1982). The media hoopla that surrounded the financial irregularities of the 1972 Nixon reelection campaign, for example, heightened people's awareness of the political and expressive benefits of Common Cause membership. Moreover, the format of the information people receive greatly influences its evaluation, a psychological phenomenon known as framing (Tversky & Kahneman, 1981). If people buy lottery tickets more readily when the purchase is described as insurance against loss rather than as a gamble on gain—as they do—people will more readily join an association when the information they receive about group membership is framed in the same way. As I noted earlier, interest groups go to great pains to cast their appeals in just that manner.

In conclusion, a theory that looks only at benefits or only at costs misses a great deal of

²This holds true if consumers' demand functions are such that relative quantities purchased do not change if income and all prices are multiplied by the same scalar. Given this assumption, the sum of the own-price elasticity (almost always negative), the income elasticity (almost always positive), and cross-price elasticities (positive for substitutes, negative for complements) equals zero. This is known in microeconomics as the homogeneity condition.

what is important. Benefits and costs do not simply exist; rather they exist in particular milieus. The effects that costs and benefits have on group membership depend upon what people know about them, upon whether people need or want the benefits, and upon whether or not they can afford the costs. In the next section, which analyzes changes in membership in three lobbies, I explore empirically the context-dependence of interest group incentives.

An Overview of the Analysis

Devising tests of interest group theories has proven far more difficult than devising the theories themselves. Two modes of inquiry predominate: appeals to anecdotal evidence and case studies of particular interest groups, based sometimes upon surveys of their members.

I take a different approach, examining the annual changes in the memberships of three groups: the American Farm Bureau Federation, the League of Women Voters, and the National Association of Home Builders.³ The Farm Bureau and the Home Builders are "economic" lobbies representing large constituencies, groups for which Olson's by-product theory is most suited. They were chosen for that very reason. The League is ordinarily thought to be a social and expressive organization; it is included to demonstrate the range of the theory.⁴

Because the theory is dynamic, the data are time series. Benefits, costs, risks, context, and membership are all constants at a single slice in time. Joining and quitting interest groups are behaviors that vary only over time, and the things that cause those behaviors vary only over time.

Because the analysis is multivariate, moreover, I can properly test a model that posits multiple causes. In each equation, my explanatory variables are environmental factors, such as actual benefits and costs, resources, and events, with which subjective benefits and costs (and

hence membership levels) should be associated, given my earlier argument. The theory predicts that membership can be expected to increase with the development of additional selective incentives. It predicts membership increases when people have higher incomes or more free time. It holds that political and economic disturbances heighten the attractiveness of political benefits, and that people consequently are more likely to join when they are threatened. When school budgets are inadequate, for example, people who value public education will rally to its support. When farmers and builders are in financial binds, they will flock to organizations that claim to have obtained vital subsidies for them. Finally, the theory asserts that group benefits are more available psychologically when the group has a high profile, so that greater visibility alone will stimulate joining. In short, the theory predicts that political and economic context affects membership because context influences subjective evaluations of benefits and costs. In turn, subjective evaluations of incentives determine membership.⁵

The American Farm Bureau Federation (AFBF)

The county agent has been the John the Baptist of the farm bureau movement.

Extension official W.A. Lloyd⁶

By any standard, the American Farm Bureau's success in recruiting members has been phenomenal. When it burst onto the scene in 1919 as a federation of state farm bureaus it was already, with 317,000 members, the second largest farm organization in the country, after the Grange (Figure 1). Like the other farm groups, its membership waned in the 1920s and early 1930s, then shot up quickly during and immediately after the Second World War. Now it is by far the largest farm group; its membership of 3.5 million dwarfs the memberships of the four other general farm organizations combined.

Two features of the Farm Bureau have figured prominently in conventional explanations of its

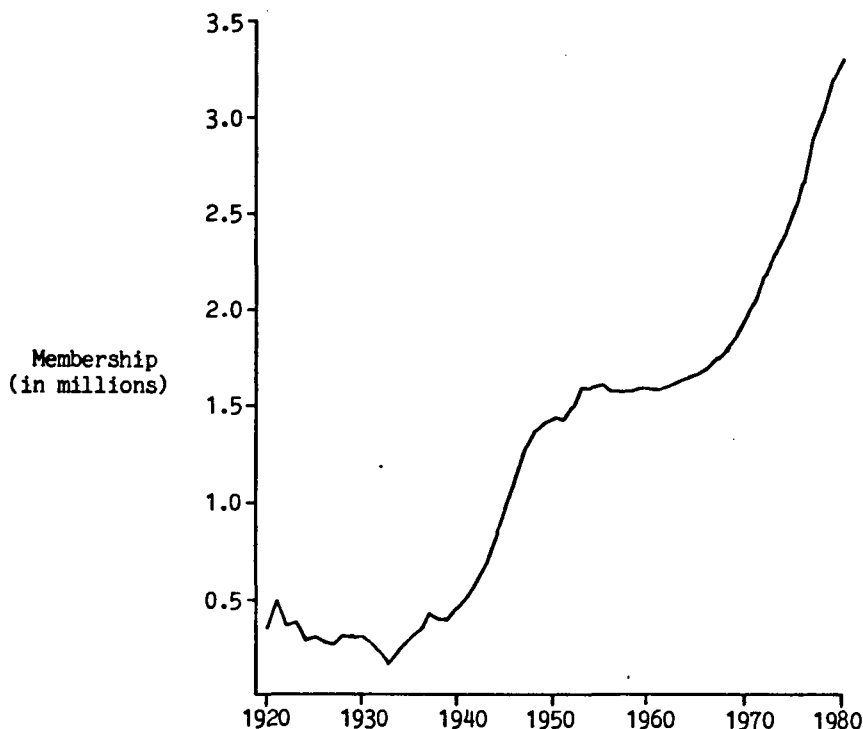
³Few aggregate time series analyses of group membership exist. See Tontz (1964) and Russell (1937) for casual analyses of farm group membership, and Ashenfelter and Pencavel (1969) (plus a comment by Mancke, 1971) for an econometric analysis of union membership.

⁴I contacted a number of organizations within the categories of farm, business, public interest, and labor groups. Most organizations (notably unions) claimed not to have past membership tallies at all, and several more declined to divulge the information. Only the Farm Bureau had already compiled figures. These three groups, then, were selected both for variety and because they were willing to release the membership data they did have.

⁵Two problems should be mentioned. First, material resources and preferences for material benefits are more easily quantified than nonmaterial resources and preferences for intangible benefits. As a consequence, my estimates are biased if the omitted intangibles are causes of group membership and are associated with the included independent variables. Second, any aggregate analysis risks committing the ecological fallacy, i.e., equating the effects of the independent variables on individuals with their effects on the population. Where possible, then, I supplement the econometric evidence with historical evidence.

⁶Quoted by Baker (1939, p. 21).

Figure 1. American Farm Bureau Federation Membership, 1920-1980



Source: American Farm Bureau Federation.

organizational success. First, to an unusual degree it was created and nurtured by a government agency, the Cooperative Extension Service (Olson, 1965/1971, pp. 148-157). The Extension organized many local farm bureaus, assisted in forming the state and national federations, recruited for Farm Bureau, and sometimes favored Farm Bureau members in allocating the county agent's services. Over time, however, the Federation's reliance upon the Extension has declined markedly. Efforts to separate the public agency from the private association began as early as 1920 and proceeded slowly (Block, 1960), but even the most hostile contemporary accounts make it clear that the present relationship is but a shadow of what it was (Berger, 1971).

Second, the state farm bureaus offer their members an impressive array of services, including everything from life insurance to farm supplies. Although nominally independent, Farm Bureau businesses are legally controlled by the state lobbying organizations (Berger, 1971, chap. 4; Olson, 1965/1971, pp. 153-156). Illinois pioneered both with insurance services and with the legal arrangement when it launched the County Mutual Insurance Company in 1925. By 1940, about a quarter of the nation's farmers,

mostly in the Midwest, could purchase insurance from Farm Bureau companies. By 1950, however, nearly 85% were in the Farm Bureau insurance trade area, owing to the incorporation of companies in the South and West.

The estimates reported in Table 1 confirm the contribution of insurance services to the Farm Bureau's growth. A four-point increase in the percentage of U.S. farms for which Farm Bureau insurance was available boosted AFBF membership more than 1% in the year before introduction, anticipating the offering, and by 1% in the year of introduction.⁷ Insurance was an even bet-

⁷In none of the equations I estimated are the dependent variables normalized, that is, standardized by dividing by the relevant population. Undeniably, they should be. Changes in the size of the potential membership pool are causes of changes in the size of membership, and if population changes are associated with other independent variables, the coefficients are biased. The simple methodological problem, however, masks a thorny theoretical one: what is the proper normalizing population? In each of my cases the answer would seem to be clear, but a moment's reflection makes the quandary apparent. Farm Bureau membership since 1977 has exceeded the number of U.S. farms; the League, which

has never considered itself a "women's" organization, has admitted men since 1972; and two-thirds of NAHB membership is drawn from "derived demand" occupations, such as architects, realtors, and bankers. Conceptually, the market is clearly bounded (all who benefit from the policies espoused by the group), but operationally the market is ill-defined and ever-shifting. One of the strategies of leaders, after all, is broadening the group's appeal. Hence, normalizing by the wrong population does not insure that the bias is avoided or obviated.

The solution offered here is, I believe, more palatable. Expressing the dependent variables as percentage changes in raw membership levels introduces a sort of normalization; it narrows the relevant comparison from all observations to adjacent observations. Thus, the estimates in the equation are unbiased if the percentage change in the size of the true normalizing population is uncorrelated with the other independent variables (few of which are themselves percentage changes). Such an assumption is not unreasonable.

The Farm Bureau equation is estimated on data that were weighted to correct for nonconstant error variance and differenced to correct for autocorrelation, with rho estimated by maximum likelihood methods. The League equation likewise corrects for autocorrelation, and the Home Builders equation likewise corrects for heteroskedasticity. Both the Farm Bureau and Home Builders data are weighted by the square root of time.

ter selling point in the year after its offering—percentage point increases in membership followed from 3% increases in coverage.

It would overstate the importance of insurance services, however, to give them the full credit for the huge increases in Farm Bureau membership after World War II. As Table 1 shows, there were other factors involved, such as political benefits, rival groups, and income. Farm Bureau insurance was available to three times as many farmers after the war, but real agricultural income per farm was also about 65% higher. Farm Bureau benefits attracted new members or retained old ones only when farmers had income sufficient to pay dues (Russell, 1937; Salisbury, 1969; Tontz, 1964). Even a casual inspection of Figure 1 shows that membership trends most parallel economic trends. The twenties and thirties were bad both for farmers and the AFBF; the postwar period was good for both. The estimates in Table 1 verify powerful income effects. Roughly a threefold increase in real agricultural income per farm produced a 6 percentage point increase in Farm Bureau membership. Membership was subjectively less costly when resources were ample.

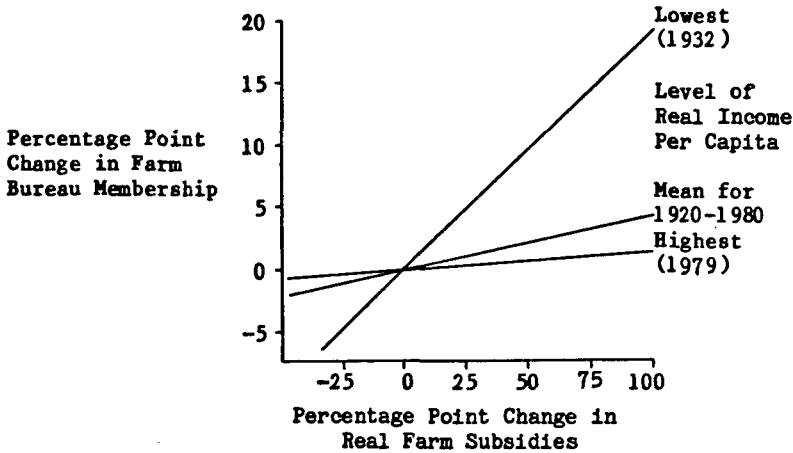
Part of the reason for the Farm Bureau's revival in the late 1930s, then, was the revival of

Table 1. Determinants of Percentage Changes in American Farm Bureau Federation Membership, 1922-1980
(Weighted maximum likelihood estimates)

	Coefficient	Standard Error
Change in percentage of farmers in a Farm Bureau insurance trade area		
($t+1$)	.30	.17
(t)	.26	.17
($t-1$)	.32	.17
Log (real agricultural income per farm in thousands of dollars)	5.59	4.10
Percentage change in real subsidy payments/ (Real agricultural income per capita in thousands of dollars)		
(t)	.14	.03
($t-1$)	.08	.03
($t-2$)	.12	.03
($t-3$)	.07	.03
Active farm protest group	-5.61	2.29
Constant	-18.82	28.65
$R^2 = .57$		
$\rho = .35$		
D.W. = 2.07		
$N = 59$		

See Appendix for data sources and definitions.

Figure 2. The Effect of Changes in Farm Subsidies on Farm Bureau Membership for Different Income Levels



Source: Table 1.

the farm economy brought about by the subsidy programs of the New Deal. But another factor in its resurgence was the AFBF's success in obtaining those subsidies. The Farm Bureau was an important—and well publicized—proponent of the 1933 Agricultural Adjustment Act (AAA), and its political success paid big dividends. As shown in Table 1, increases in agricultural subsidy payments boosted Farm Bureau membership then and for several years after, but the magnitude of the increase was contingent upon the level of farm income. When income was at its lowest point, each additional 5 percentage point current-year increase in farm subsidy payments boosted AFBF membership by 1 percentage point (Figure 2). When income was at its highest point, however, it took nearly a 100 percentage point increase in government payments to produce a percentage point increase in AFBF membership. The response to the Federation's political benefits, in short, depended upon the seriousness of the economic threat.

Of course it is impossible to tell from aggregate estimates whether joiners really were responding to political benefits. They might, for instance, have been attracted by expressive incentives. The history of the Farm Bureau, however, argues against that possibility. Although farmers have been prone to expressiveness, the AFBF has not. Even while lobbying aggressively for fundamental changes in agricultural policy, the AFBF was cool, if not hostile, toward all radical agrarian groups from the Nonpartisan League to the American Agriculture Movement. Expressive benefits have not been absent, of course. During

the New Deal, Farm Bureau rhetoric emphasized that parity prices meant "equality for agriculture," and one critic has characterized its recent ideology as "right-wing in overalls" (Berger, 1971, p. 5). By and large, however, farmers seeking expressive benefits have had to look elsewhere.

In fact, the expressively motivated farmers have looked elsewhere. Farm Bureau membership dropped by over 5 percentage points in years when the Farmers Holiday, the National Farmers Organization, and the American Agriculture Movement were active and visible (Table 1). The Farm Bureau simply did not provide the expressive benefits that farmers often sought, and when other groups did provide them, many substituted protest group activity for AFBF membership.

A more formidable alternative interpretation of the finding has it that payments under the AAA were seen by farmers not as a collective good but as a selective benefit contingent upon Farm Bureau membership. The Extension Service initially administered the AAA, and Olson (1965/1971, pp. 151-152) argues that the Farm Bureau's influence over the Extension enabled it to coerce subsidy recipients into joining. County agents "encouraged" farmers to enlist with local farm bureaus, implying that those who failed to do so would notice the difference in their checks.

The historical record indicates, however, that an explanation emphasizing collective goods cannot be dismissed so easily. Two elements of Olson's empirical argument need revision. First, the administrative authority of the Extension Ser-

vice *and* the influence of the AFBF over the Extension varied by farm program and by region. County-agent control over administration was greatest in areas where Farm Bureau strength was least and vice versa. The Extension possessed the most authority in the tobacco, cotton, and peanut programs of the South (Baker, 1939, pp. 75, 94), but during the thirties the Southern farm bureaus were relatively small and weak and often subservient to, rather than dominant over, the Extension services (Baker, 1939, p. 141). The influence of the farm bureaus over the Extension was greatest in the Midwest, but there the wheat and corn programs were administered mainly by committees elected by local farmers. This did not, of course, preclude Farm Bureau influence. In fact, an estimated 85% of AAA committeemen in New York, Illinois, and Iowa (all AFBF strongholds) were Farm Bureau members (Baker, 1939, p. 73; Kile, 1948, p. 205). My point here is not that coercion was insubstantial. Rather, it is that the potential ability to coerce was greatest in regions where Farm Bureau had the least to gain from it.

Second, and more important, the Extension did not administer the farm programs for very long. After the passage of the 1936 Soil Conservation and Domestic Allotment Act,

responsibilities for local administration were withdrawn from the county agents of the Extension Service. . . . The [Agricultural Adjustment Administration] had its own county offices and its own farmer committeemen. While the Farm Bureau was in some cases able to dominate the farmer committeemen of the A.A.A., the influence of the Farm Bureau with a centralized agency was not likely to be as strong as with a decentralized agency like the Extension Service, since the organization and development of the Farm Bureau virtually paralleled that of the Extension Service (Campbell, 1962, p. 157; Block, 1960, pp. 16-17).

The replacement of one bureaucracy with another makes no appreciable difference to the quality of the model; the equation predicts membership changes for 1937 to 1940, the prewar years during which the AAA administered the subsidy programs, just as well as for 1933 to 1936, when the Extension possessed the authority.⁸

The AAA subsidy programs were important for two other reasons. First, the AFBF feared that the local committees set up by the farm programs might be organized into a rival interest group, especially in the South. Hence, starting in 1935, it waged an aggressive campaign to enlist Southern committeemen as farm bureau members and

leaders (Campbell, 1962, chap. 6). The informational (and, perhaps, coercive) campaign paid off—membership growth in the South in the 1930s outpaced growth in all other regions. Second, the New Deal farm legislation was popularly associated with the Farm Bureau, the AFBF encouraged that association, and farmers joined out of responsibility or gratitude or simply to insure the flow of benefits in the future. “In states where the Farm Bureau had an active program,” Baker (1939, p. 7) reports, “it claimed considerable credit for the enactment of the AAA. In these states, non-farm bureau members often felt a moral obligation to become members and promoters of the county farm bureau organization.” (See also Campbell, 1962, p. 63.) The benefits that produced the aggregate relationship were not wholly selective, as Olson asserts, but also were collective. Political success and high visibility combined to boost Farm Bureau membership growth past the level determined only by income and selective benefits.

Similar circumstances may have occasioned the Federation’s best recruiting year ever, 1921. By all accounts, membership should have dropped. Real income was off about 20% from 1920. More important (from Olson’s standpoint), Dr. A.C. True of the States Relations Bureau (precursor of the Extension Service) and AFBF President J.R. Howard had concluded a “memorandum of understanding” that forbade county agents from recruiting for Farm Bureau.

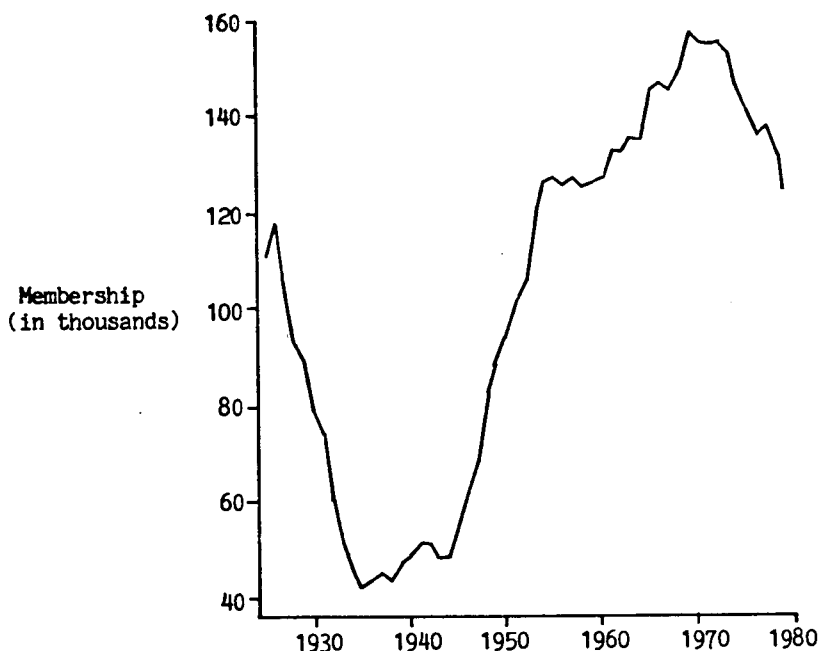
Instead of falling, though, membership jumped by 50%, AFBF’s largest percentage gain ever.⁹ May 1921 saw the creation of the infamous Farm Bloc in the Washington office of AFBF lobbyist Gray Silver. By the end of the year, the Farm Bloc had passed six priority pieces of legislation, including bills regulating meat packers and the grain exchanges (Kile, 1948, chap. 7). The Federation’s legislative clout was no doubt an important selling point in the recruitment campaign the new organization undertook (Kile, 1948, chap. 5).

And what of the True-Howard Agreement? Clearly, if the memorandum had any effect at all, it was not immediate. Farm Bureau membership began to drop a full year after its signing. Of course, in August 1922 the agreement was reissued as USDA policy, and this separation of the Farm Bureau and the Extension Service may have pro-

⁹Because of the differencing used in autocorrelation correction procedures, the coefficients for 1921 and 1922 dummies cannot be reported. Weighted least squares estimates indicate a 52 percentage point increase in membership in 1921 over what would be expected on the basis of other variables in the equation. The 1922 decline not accounted for by lower income, on the other hand, is only 17 percentage points.

⁸The mean residual for 1933 to 1936 is 5.0. The mean for 1937 to 1940 is 3.0.

Figure 3. League of Women Voters Membership, 1925-1980



Source: Compiled from League of Women Voters records and archives.

voked the 1922 membership decline, but it is doubtful. Neither edict, it is clear, changed long-standing practice. The separation of the Extension and the Farm Bureau proceeded slowly and by starts (Block, 1960), and as late as 1954 the USDA saw fit to reprimand the county agents for performing Farm Bureau tasks (Berger, 1971, chap. 8). The importance of the True-Howard Agreement has been overstated.¹⁰ Farm Bureau membership declined in 1922 and throughout the twenties because of a long and severe farm depression.

Even under the most cautious interpretation, then, this analysis of Farm Bureau membership is solid in its support of the theory. Political success begot membership success, especially when farmers were threatened. But the Farm Bureau's influence over the Extension is a compelling alternative explanation that cannot, and should not, be dispelled totally. This ambiguity is not present in the two remaining cases.

The League of Women Voters (LWV)

What does the average member want when she joins the League? . . . She wants to feel that the *League as a group* is effective in doing something to improve the governmental situation. . . . She wants sociability(?)[sic].

From materials prepared for the 1938 LWV Board of Directors meeting.

The League of Women Voters is a social and political organization nonpareil. Organized in 1920 at the victory convention of the National American Woman Suffrage Association, the League offers only one tangible selective incentive—policy study groups to inform its members on a broad range of political issues. As the memorandum quoted above and their own surveys indicate, its primary benefits are political and social (Cantril & Cantril, 1974, pp. 7, 15).

As a consequence of its reliance on intangible incentives, League membership has been particularly sensitive to changes in income and in fashion (Figure 3). Its membership remained high during the early 1920s but began to drop in the latter part of that decade as the enthusiasm of the suffragette days wore thin. Then came the Depression—at its nadir in 1935 League membership stood at only

¹⁰Part of the confusion stems from unfamiliarity with the time frame. The agreement was signed in April, 1921, but AFBF membership figures are current as of November 30.

40% of its total in the early twenties. But membership revived with prosperity, and only recently has it again begun to lag. Since the late 1960s, the League has had to compete with careers and with new women's groups.

Women's entry into the workforce had contradictory effects on the League. On the one hand, jobs boosted the discretionary incomes of upper-middle-class women, making membership more affordable. On the other hand, jobs provided many of the same benefits as League membership—sociability and intellectual engagement—and consumed time, a resource especially precious to the highly participatory League. The estimates in Table 2 reflect the contradiction. A percentage point increase in the workforce participation rate for females decreased LWV membership by 1.5 percentage points a year later. Two years later, however, the effect of a percentage point increase was the opposite, but not offsetting—a percentage point increase in LWV membership.¹¹ For the League, then, the coincidence of the influx of women into the workforce and the creation of the National Organization for Women (NOW), the National Abortion Rights Action League (NARAL), and other women's groups was doubly debilitating. The existence of both career and group substitutes increased the price (in this case, time) elasticity of demand for LWV membership. Because jobs produced greater discretionary income, the new women's groups, which made greater monetary demands but lesser time demands, were aided at the League's expense. A change in the relative availability of different

resources affected the relative attractiveness of groups with different cost structures.¹²

The League's decentralized, participatory character, though, made it better able to provide an important collective benefit. In many communities, the League of Women Voters was most prominent as a lobbyist of school boards.¹³ Quality of public education was just the sort of highly salient issue that could mobilize educated, upper-status women, many with school-aged children of their own. The League's 1974 self-study found, for example, that one and a half times as many women with children at home were very active in local leagues as women without children at home (Cantril & Cantril, 1974, p. 5). League involvement in school politics, moreover, was invariably directed toward *defense* of existing educational services, and relative to the more specialized teachers' unions and parent-teachers associations, it was rather episodic. Zeigler and Jennings (1974, p. 117) conclude, for example, that "non-issue-specific groups, such as the . . . Leagues of Women Voters, . . . provide support for the ongoing system, but inject little conflict. . . . They constitute a resource from which decision makers may draw in times of crisis." Accordingly, my estimates indicate that LWV membership grew by a percentage point when per capita state and local support for public education fell by \$10, in other words, when support for public education was inadequate. In some measure, then, membership in this national federation reflects neither attention to national affairs nor responsiveness to selective inducements, but

¹¹The equation is estimated on only part of the data available because yearly workforce participation rates for females do not exist before 1940. Omitting this variable for the period from 1925 to 1940 would introduce a serious specification error. Moreover, unlike the membership change series for the Farm Bureau and Home Builders, the membership change series for the League is not stationary. Hence, the equation was estimated on differenced data to insure that the patterns of association found here are not the result of both the dependent variable and the independent variables exhibiting parallel trends (this is in addition to the correction for autocorrelation).

Differencing also reduces drastically the collinearity between school expenditures and disposable income (aux. $R^2 = .39$), allowing their effects to be distinguished. But even here, disposable income has no effect on changes in LWV membership—while the coefficient is positive, it is dwarfed by its standard error ($p = .55$). The problem would seem to be the inability to measure adequately the competition from new women's groups in the 1970s. This omitted variable, which is associated with high levels of disposable income, probably biases downward the estimate of the income effect.

¹²Entry into the workforce and the founding and growth of NOW were, of course, both a consequence and cause of rising women's consciousness, and the connection between workforce participation and League membership may be as much the result of the relatively greater attractiveness of NOW benefits as of price advantages. It is clear, however, that as of 1970 few women had dropped out of the League to join other women's groups but that dropouts were more likely to be holders of full-time jobs (Cantril & Cantril, 1974, pp. 12-13). Although the League appears to have lost members as a result of its equivocal stance on the Equal Rights Amendment, the competition between LWV and NOW seems to be for women who were not members of any women's groups. The argument here is that many of these women would have joined the League had NOW and NARAL not been available. League membership held its own in the 1940s, when women entered the war economy in large numbers but no alternative women's political groups existed. The most recent LWV membership decline, however, dates from 1969, which coincidentally or not is only three years after NOW's founding.

¹³The importance of local educational issues to the League, which did not initially occur to me, was emphasized by a staffer in the LWV's membership division.

Table 2. Determinants of Percentage Changes in League of Women Voters Membership, 1946-1980
(Maximum likelihood estimates on differenced data)

	Coefficient	Standard Error
Change in female workforce participation rate		
(<i>t</i> - 1)	-1.63	.32
(<i>t</i> - 2)	.97	.31
Real per capita state and local expenditures on public schools (in dollars)	-.11	.05
Presidential election years	4.90	.82
Constant	.69	.59
$R^2 = .67$		
$\rho = -.35$		
D.W. = 2.22		
$N = 35$		

See Appendix for data sources and definitions.

rather the great salience of the political benefits offered by its locals.

The LWV's voter education and get-out-the-vote crusades naturally were no less important, but primarily because of the publicity and excitement they generated. The League has been most active, and most visible, in presidential election years. In the 1928 Hoover-Smith contest, for example, it sponsored a weekly series of pre-election radio programs, and in 1976 and 1980 it staged the presidential debates. The favorable light these events cast on the LWV boosted membership by about 5 percentage points.

Two lessons emerge, then, from this analysis of the League of Women Voters. First, League membership responds to the same stimuli to which Farm Bureau membership responds—changes in resource availability and visibility—even though the League relies more on intangible incentives. Second, threats to valued collective goods mobilized League members to the same extent as threats mobilized farmers. These conclusions apply equally strongly to the final case I consider, the National Association of Home Builders.

The National Association of Home Builders (NAHB)

[NAHB members] have a tendency to swing erratically and sharply with the trends in homebuilding: When building drops, then they get radical; but when times get good and they get fat, then they go conservative.

A National League of Cities-U.S. Conference of Mayors official ¹⁴

The years after World War II brought unprecedented prosperity to the residential construction industry. As a result of forced savings during the war, federal loan and tax subsidies, population growth, and affluence, the stock of owner-occupied housing increased 246% between 1940 and 1980, compared to a population increase of only 72%. The National Association of Home Builders rode the crest of the postwar housing boom (Figure 4). Founded in 1942, NAHB grew spectacularly from its original 500 members to its current 124,000, most organized into local chapters. Only one-third are builders, "anyone who at any point in his lifetime has constructed one home for sale" (Lilley, 1971, p. 444); the majority are associates—subcontractors, architects, realtors, banks, and other suppliers.

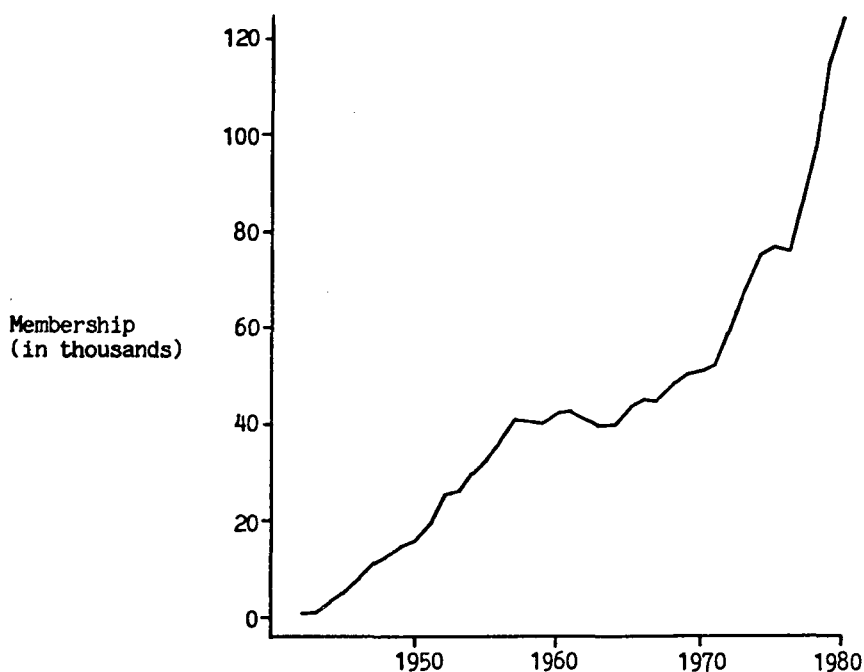
The estimates in Table 3 confirm that money in builders' pockets meant builders and suppliers on NAHB rolls and cash in NAHB coffers. A 2-billion-dollar increase in aggregate industry income upped membership by over a percentage point.¹⁵

But if prosperity was the road to organizational success for the Home Builders, it was a bumpy, uncertain ride. Federal monetary manipulations designed to stabilize the economy fell especially hard on the interest-rate-sensitive residential con-

¹⁵Unlike the independent variables in the Farm Bureau and League equations, the explanatory variables in the NAHB equation are not normalized. The Census Bureau stopped tabulating the number of firms in contract construction in 1963. Estimation of the equation with normalized independent variables for those 20 years, however, demonstrates only one specification change—the coefficient for construction income fails to attain significance.

¹⁴Quoted by Lilley (1971, p. 445).

Figure 4. National Association of Home Builders Membership, 1942-1980



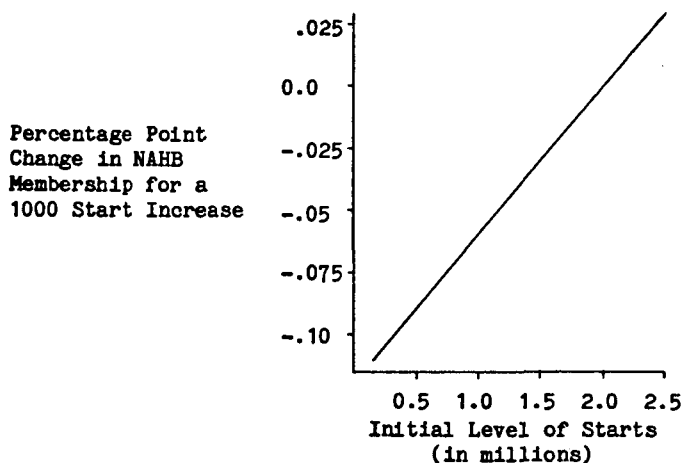
Source: Compiled from National Association of Home Builders records.

Table 3. Determinants of Percentage Changes in National Association of Home Builders Membership, 1943-1980
(Weighted least squares estimates)

	Coefficient	Standard Error
Real residential construction income in billions of dollars	.60	.17
Housing starts in thousands	.12	.02
Housing starts (in thousands) squared ($\times 1000$)	.30	.07
Housing starts under FHA insurance or VA loan in thousands/(Real residential construction income in billions of dollars)	4.41	.73
Constant	49.7	20.3
$R^2 = .87$		
D W. = 2.06		
$N = 38$		

See Appendix for data sources and definitions.

Figure 5. The Effect of Changes in Housing Starts on National Association of Home Builders Membership



Source: First derivative, with respect to starts, of equation reported in Table 3.

struction industry. Because building activity fluctuated widely, future prospects were always uncertain. Accordingly, when housing starts were below about 2 million annually, NAHB growth was greatest when starts were low, but for new construction activity above that level, growth was greatest when starts were high. At the lowest level of building activity, a decrease of 10,000 starts produced an NAHB membership increase of a percentage point (Figure 5). At the highest level, a decrease of 10,000 produced a small membership drop of approximately .2 of a percentage point.

The nonlinearity results, I suspect, from a difference between the type of firms that populated the industry in good times and the type that remained in bad times. When building activity and potential profits were high, new firms, which tended to be smaller and undercapitalized, entered the industry. In bad times, however, these marginal firms exited, leaving the large-volume, well-capitalized operations. Thus, decreases in building activity when it already was high mainly affected the small, low-volume builders, who, even though threatened, lacked the accumulated resources to pay dues. Decreases in starts when activity already was low affected large builders, who had resources even in hard times. Housing starts for builders, like educational expenditures for upper-status women, were a psychological indicator of well-being, an indicator to which large builders were better poised to respond.

Of course if homebuilders' woes stemmed from policy decisions made by the Federal Reserve Board, there clearly was little that a lobbying organization could do to help. The alternative was

to seek help elsewhere, in the bureaucracies that administered the most important and extensive housing subsidy programs, federally backed Federal Housing Administration (FHA) and Veterans Administration (VA) loans. "FHA," as NAHB's chief executive lobbyist put it, "is our major point of contact with the federal bureaucracy" (Lilley, 1971, p. 441). Even in good times the loan subsidies were of considerable importance: "NAHB membership is responsible for a [significant] percentage—90 percent or higher—of houses built under some government insurance or subsidy program" (Lilley, 1971, p. 433).

The effect of realized building subsidies on NAHB membership paralleled the effect of realized farm subsidies on AFBF membership. As shown in Table 3, the value of the collective benefits depended upon builders' incomes. The effect was greatest when income was low. When income was least adequate, a rise of only 1000 government-backed starts swelled NAHB rolls by 1 percentage point (Figure 6). When income was at its highest, the same percentage point membership increase required an additional 25,000 federally subsidized starts. The home builders did indeed get radical in hard times; they did indeed go conservative when they got fat.

As before, of course, the finding may reflect an associated selective benefit provided to members. Although FHA and VA guidelines are legislated, the agencies decide who meets the loan criteria and who does not. If the Home Builders were able to intercede on behalf of particular builders' customers, preferential treatment might constitute sufficient explanation.

Not only does such a contention lack supporting evidence, it is also implausible. NAHB does lobby FHA on behalf of individual members, but the thrust of such efforts is settlement of disputes over building code requirements for FHA-insured houses (Lilley, 1971, p. 441). The FHA and VA bureaucracies are simply not as open to NAHB influence as the decentralized Extension Service was to the Farm Bureau.

A more plausible explanation is that the Home Builders could at least put on a show of pressuring FHA and VA. As an NAHB membership division staffer commented to me, when the industry is emerging from a recession, "NAHB benefits if it has fought hard to end the crunch." FHA, like its parent bureau of Housing and Urban Development (HUD), "is renowned for slowness bordering on catatonia" (Headey, 1978, p. 193; Weicher, 1980, pp. 113-114). The Association of Home Builders could press FHA and VA to speed processing of loan applications so that builders could build.

Until recently, moreover, the loan insurance and subsidy programs were "off budget." Hence, faster processing and more lenient evaluations of loan applicants were perhaps a handy means by which the federal government could compensate the construction industry for the hardships it suffered as a result of anti-inflation policy. Whether NAHB pressure was responsible for the subsidies or not is immaterial. All NAHB had to do was claim the credit for it. Like farmers joining the Farm Bureau, builders joined the Home Builders out of gratitude, responsibility, or to insure that the flow of benefits was not interrupted.

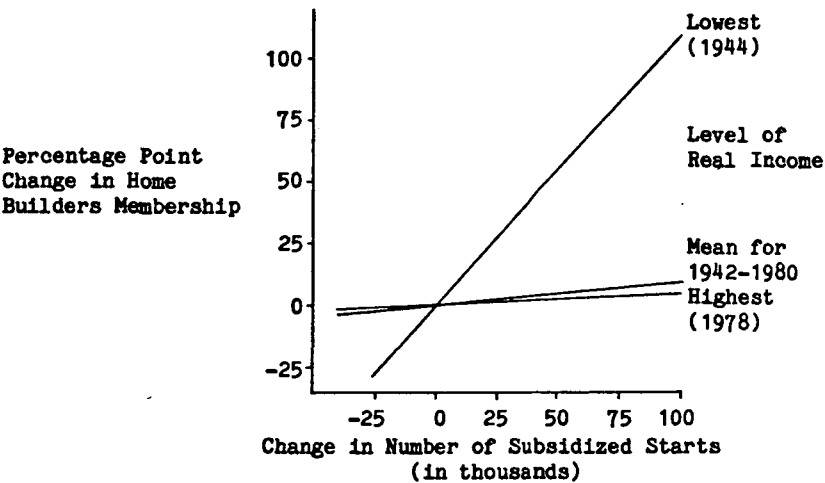
The National Association of Home Builders, in sum, presents a strong case: mobilization in response to threats and mobilization in response to political benefits offered in times of stress.

Discussion

Interest group incentives do not exist in a vacuum. As my three case studies have shown, incentives have different effects in different contexts. Two consistencies are especially important. First, political benefits matter. Although the case of the Farm Bureau is murky because of its influence over the Extension and over local committeemen, the case of the Home Builders is not. Membership responded to realized subsidies, a collective benefit. Second, political benefits matter most when groups are threatened. Subsidy benefits interacted with lower income to swell AFBF and NAHB rolls. Inadequate expenditures on public schools stimulated League growth. Contrary to Olson's claim, then, individuals do indeed join interest groups in response to collective benefits.

My conclusion, however, sets aside a complementary question, that of supply. It makes little difference whether or not members of large groups respond to collective incentives if associations representing large groups never come into being; that is, for individuals to respond to political incentives (or to *any* incentives) they first must exist. Developing membership benefits is extremely costly, and the larger the group, the more costly the organizing endeavor. For a group even to be organized, therefore, it must be subsidized

Figure 6. The Effects of Changes in Housing Subsidies on Home Builders Membership for Different Income Levels



Source: Table 3.

by entrepreneurs, by other groups, or by governments.

The key to understanding which organizations form, then, is understanding which groups get subsidized and when. Producer groups predominated early in the twentieth century, for example, because of smaller size and easier access to organizing resources. The concentration of U.S. industry insured numerous small groups and monopoly rents, that is, lower initial costs and greater resources. Producer groups, moreover, held a privileged position with government (Lindblom, 1977, chap. 13). Political leaders needed the support of business and hence were often eager to create groups as a conduit for communication. The greatest impetus to business (and labor) organization, for example, was war, when government most needed to secure the concurrence of producers. During World War I the federal government encouraged cooperation among firms in vital war industries (McConnell, 1966, pp. 60-64) and hastened the creation of local farm bureaus by the States Relations Bureau in an effort to boost food production (Baker, 1939, p. 44; Kile, 1948, p. 42). Finally, producer groups were more likely than consumers to call forth entrepreneurs willing to bear initial costs. Miller et al. (1981) have documented the importance of group identification in predicting other forms of participation. Part of the advantage of producer groups over diffuse groups, then, was more natural identification with the former and hence greater responsiveness to intrinsic, group-centered rewards such as solidary and expressive benefits.

The proliferation of consumer and environmental groups in the 1970s reflected an expansion of subsidization. Foremost among the developments was the growth of charitable foundations, the bread-and-butter of the public interest sector (Berry, 1977, pp. 72-74; Walker, 1983). Second, government programs in the 1960s often mandated or encouraged the creation of countervailing groups. And finally, entrepreneurs' consciousness of membership in more diffuse groups preceded that consciousness among the masses. Nader's experience with the Corvair certainly alerted him to the marketability of consumer crusading, but it probably also made him more willing to subsidize the consumer movement for its own sake.¹⁶

¹⁶Clearly, many large groups organized by entrepreneurs will fail, but one cannot conclude from that fact alone that rational individuals will not undertake organizing endeavors (Hardin, 1982, pp. 36-37). We commonly presume, after all, that economic entrepreneurs are rational, yet each year many thousands optimistically launch small businesses even though 62% will fail within four years (Small Business Administration, 1983).

There is, in sum, a Say's law of interest groups: supply creates its own demand. Individuals can only respond to whatever incentives are there. Subsidization does not end with creation, however; it extends even to maintenance. In a sense, politics subsidizes services. Interest groups' service organizations quite ironically are by-products of their lobbying organizations, not the other way around. In all but a few cases, services were added to political benefits, not political benefits to services (Hardin, 1982, p. 34). Interest groups entered into competition with commercial firms. If service markets are competitive and if interest groups and commercial companies face similar cost structures, no service provider can undercut the market price, and the only competitive avenue left is product differentiation. In addition, out of the many firms that offer a particular service, *only interest groups offer political benefits in addition*. Part, therefore, of the politically induced changes in interest-group membership levels stems from competitive advantage.

One can, then, identify three classes of joiners motivated by political benefits. For some people, political benefits are sufficient of themselves. Others join both for services and for policy, but either alone is insufficient (Moe, 1980, p. 607). For still others, political benefits are the crucial quality difference; they make the difference between buying from the interest group and buying from a commercial firm. In each case, political benefits are the pivotal component because they subsidize services. Far from being a marginal phenomenon, therefore, responsiveness to political incentives is more widespread and noteworthy than heretofore believed.

Two implications follow. First, the organizational prospects for interests common to large groups are not nearly so bleak as critics of group theory have asserted. On the other hand, because start-up costs are related directly to group size, subsidization is essential for organization. In a sense, this conclusion is more pessimistic even than Olson's, pointing as it does to the explicitly political calculations that give rise to group formation. If certain associations do not exist, it is because it was not in the political interests of resource holders to put them there. That is not to say, of course, that subsidization is the *sine qua non* of organizational success. Demand for interest group benefits must also exist. Perhaps this seems equivocal; that is, to argue that the high risk of failure among expressive groups, to take one instance, results from the absence of effective demand. But groups offering material services fail or lose members as well. The Farmers Alliance, for example, offered a variety of farm services but collapsed when the 1893 depression undercut its members' buying power and the People's Party

stole its political program. In short, there are no sufficient conditions for interest group formation and maintenance, only a host of necessary—and highly contingent—conditions.

Appendix: Data Sources

Unless otherwise noted, the data were drawn from *Historical Statistics of the U.S. from Colonial Times to 1970* and updated from the *Statistical Abstract of the United States*, both published by the U.S. Commerce Department. Real money series are in 1980 dollars, and all were deflated by the personal consumption price deflator. The deflator for 1920 to 1970 is drawn from *Long Term Economic Growth, 1860-1970*, a U.S. Commerce Department publication (1973), pp. 222-223, and updated from the *Statistical Abstract*.

American Farm Bureau Federation membership as of November 30, 1920-1982: Courtesy of the American Farm Bureau Federation.

League of Women Voters membership as of approximately January 1, 1925-1982: Compiled from League records and archives by the author.

National Association of Home Builders membership as of January 1, 1942-1982: Compiled from NAHB records by the author.

Agricultural income: 1929-70: National income from agriculture, forestry, and fisheries, Series F-227. 1921-28: Robert F. Martin, *National Income in the United States, 1799-1938*. New York: National Industrial Conference Board, p. 65.

Direct subsidy payments to farmers: Series K-326. When expressed as a percentage change, 1933, the year payments commenced, is coded zero.

Farm population: Series K-1.

Number of farms: Annual: Series K-4. Quinquennial, by state: Series K-17 to K-81.

States with Farm Bureau insurance companies: *Farm Bureau Insurance*, pamphlet, American Farm Bureau Federation, 1981; *Best's Insurance Reports: Property/Casualty, Life/Health*. Oldwick, N.J.: A.M. Best Co., 1983; Kile, 1948, pp. 346-352; Berger, 1971, chap. 4. States were considered to have Farm Bureau insurance from the date at which the company currently offering either property and casualty or life insurance (whichever was earliest) was founded. Because one company often serves several states, this estimate probably errs toward crediting states with an insurance program before they actually had one. For other states, however, it errs toward lateness, because many contracted for insurance through the Nationwide Insurance Company (an Ohio Farm Bureau affiliate) before forming their own companies.

Active farm protest groups: A dummy variable for the Farmers Holiday Association strikes and penny auctions (1932 and 1933), the National Farmers Organization protests and holding actions (1956-1962, 1964, 1967 and 1968), and the American Agriculture Movement tractorcades (1978 and 1979).

State and local expenditures for public schools: Series Y-686. Data are annual for 1952-1980 and are interpolated from biannual data for 1924-1952.

Labor force participation rate for females: Series D-36. Housing starts: Series N-156.

New privately owned housing units started under FHA insurance or VA guarantee: Series N-180 and N-181.

Residential construction income: Value of new residential construction put in place, Series N-3.

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Aggregate Stability and Individual-Level Flux in Mass Belief Systems: The Level of Analysis Paradox

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Converse's findings of low constraint and stability among mass attitudes are only one side of the story; mass attitudes often manifest much more coherent and stable patterns at the aggregate level than would seem possible if one took the results of panel survey analysis at face value. Items designed by Rokeach and Inglehart to tap basic value priorities, show modest individual-level stability, together with remarkably high aggregate stability structured in ways that could not occur if random answering were the prevailing pattern. Materialist/postmaterialist values show large differences between birth cohorts that not only persisted throughout 1970-1984 but seem to reflect distinctive formative experiences that occurred as much as 50 years ago. These aggregate results are much too skewed to result from equiprobable random answering and cannot be attributed to methods effects. They reflect underlying attitudinal predispositions in the respondents themselves. While random response to given items does play an important role, it is much less widespread than Converse's Black and White model implies and does not generally reflect an absence of relevant preferences. Structural equation analysis of multiple indicators demonstrates much stabler, broader orientations underlying the response to given items that account for the high aggregate-level stability observed here. Because it usually measures such orientations imperfectly, individual-level survey data tend systematically to underestimate constraint and stability in mass attitudes.

The development of behavioral research in the postwar era provided disturbing evidence about the coherence and stability of mass political orientations which has haunted us ever since. A central role was played by Converse (1964, 1970), who found that the political ideas of the American public generally display surprisingly little struc-

ture and seem to fluctuate capriciously over time. Converse's analysis of panel survey data from 1956, 1958, and 1960 showed that on the average, less than two-thirds of the American public took the same side on important political issues at both time points in a two-year period. Because approximately 50% would do so by chance alone, public attitudes seemed dismayingly unstable.

The implications of these findings were disturbing. To many people they seemed to imply that representative democracy was an unattainable myth. Under democratic norms, public officials are supposed to implement policies that reflect the preferences of the majority of citizens, but if most citizens don't really *have* any coherent or stable preferences about major political issues, why should political decision makers take them into account? Indeed, how *could* they?

In some quarters, Converse—the bearer of unwelcome news—was even depicted as an advocate of elitism. Although the charge is clearly unwarranted, the implications of his findings are troubling indeed. They have given rise to a wide-ranging, often heated debate (Achen, 1975; Converse, 1974, 1980; Erikson, 1978; Jackson, 1983; Judd, Krosnick & Milburn, 1982; Judd & Milburn, 1980; Kinder, 1983; Martin, 1981; Pierce & Rose, 1974).

The debate continues: In 1983, Converse's (1964, 1970) original articles were cited in literally scores of publications, some agreeing and others

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disagreeing. There are several reasons why this controversy has been so enduring. The first reason is the powerful normative implications; it raises the question of whether or not representative democracy is meaningful or even possible if non-attitudes are as widespread as Converse's analysis implies. Second, it raises the basic epistemological question of survey research: Can we trust the evidence of our measuring instruments? The findings suggest that survey evidence is mostly noise. Converse concluded that more than three-quarters of the public were giving random answers to the question used to illustrate his Black and White model. He never claimed that this held true of all political attitudes, of course, but the impact of his analysis stems from its implications concerning the nature of belief systems in mass publics, not the stability of one particular item. One commonly finds continuity correlations as low or lower than that found with the item used in the Black and White model, and when one does, the model implies that the overwhelming majority have no real preference and are giving random answers. If this is true, it is important and alarming information for anyone involved in survey research.

There is a third reason why the debate has been so enduring; both sides are partly right. As I will argue in this article, Converse was right; there is a major component of random answering in most survey data. Moreover, it can be traced to the respondents themselves—it is not simply an artifact of poor questionnaire construction (although vague or confusing questions would increase it). Random answering exists, but it is not nearly as widespread as Converse's analysis indicates, and it is not located where he placed it, concentrated entirely in one of two radically different types of respondents. Instead, the evidence presented here indicates that most respondents *have* preferences that are relevant to most important social issues, but it is difficult to measure them accurately, given the constraints of time and motivation that normally characterize survey research. As a result, survey data tend systematically to underestimate both stability and constraint in mass attitudes.

In short, Converse contributed an immensely important insight. Survey data are not nearly as hard as they would seem if one took all marginal distributions at face value. But this insight must be supplemented by another one, growing out of a cumulative body of research: survey data are not nearly as soft as they would seem, if one took the turnover displayed in a typical panel survey at face value. Instead there are underlying elements of stability which can be discerned when one applies more refined measurement techniques and which manifest themselves directly when one examines certain types of survey data at the ag-

gregate level, as I do in this article.

Figure 1, which depicts three different models of attitudinal stability, illustrates the argument. The first model is a Markov Chain model of the type that Converse (1970) starts with in his analysis of attitudes and nonattitudes. Using this model, let us assume a correlation of .40 between the public's issue preferences at time 1 and the same individuals' preferences on the same issue at time 2, two years later; similarly, there is a .40 correlation between preferences at time 2 and preferences at time 3, four years later than time 1. The broken arrow indicates the correlation that would exist as a result of these two causal linkages. Assuming that no other factors are involved, this model implies that there is a correlation of .16 between preferences at time 1 and preferences at time 3 ($.4 \times .4 = .16$); one would expect a pronounced decline in the correlation as the time interval gets longer.

But the findings Converse actually observed in his 1956-1958-1960 panel survey deviated drastically from this pattern. The correlations from time 1 to time 2, and from time 2 to time 3, were both approximately .39, but the correlation from time 1 to time 3 was also about .39. The correlations over two years were not very high, but they showed no decline when extended over longer periods of time.

These findings were astonishing because they seemed to violate normal causal principles. In order to resolve this paradox, Converse developed the Black and White model depicted in Figure 1. The observed phenomena could be explained if one assumed that a large share of the sample (80%, in this case) had no real preferences, or nonattitudes, and were responding literally at random. Because their responses were random, they would show no correlation whatever from one time to the next, regardless of whether the time intervals were long or short. The remaining respondents' behavior was totally different (hence, the "Black and White" label for this model). This second group not only had real preferences, but their preferences were totally stable; regardless of how much time elapsed, the correlation remained a perfect 1.00. For completely different reasons, the attitudinal stability of both groups shows no change over time. Thus, when the two groups were mixed together in a representative sample of the public, one would observe an overall continuity correlation of only .39 from one survey to the next, but the correlation would not decay over time.

This model fit the data neatly, explaining some findings that otherwise seemed astonishing. But the Black and White model is problematic. Apart from its alarming normative and epistemological implications, it is inherently implausible, because

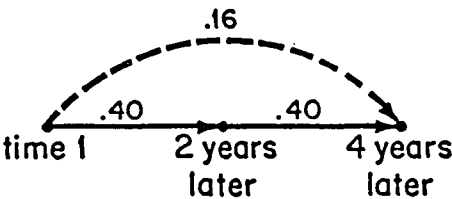
it postulates that the public is dichotomized into two radically different types of respondents—a large group of apoliticals and a small group of rigid ideologues, with nothing in between. A large body of survey research, including some of Con-

verse’s own work, suggests that this is unlikely—the public normally falls at various points along a continuum, rather than being polarized at the extremes.

The third model in Figure 1 provides an alter-

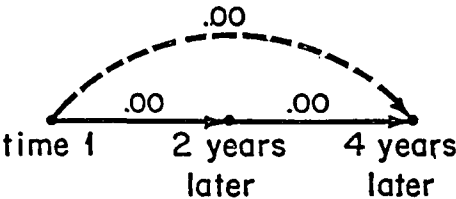
Figure 1. Three Models of Continuity and Change in Mass Attitudes

1. Markov Chain model:

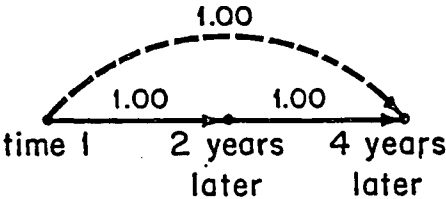


2. Black and White model:

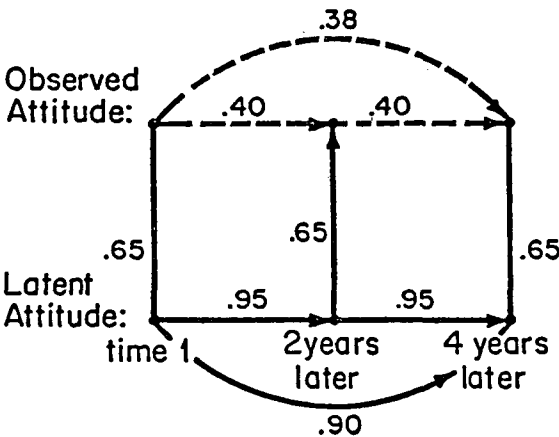
(a) 80% of sample



(b) 20% of sample



3. Latent Attitude model:



native explanation. This model is based on the assumption that practically everybody has relatively stable underlying preferences that shape one's responses to important political questions, but that any given survey question measures them imperfectly. If, as in the example shown here, the latent attitude has a stability correlation of .95 over each two-year period, but there is a correlation of .65 between the observed attitude and the latent attitude at any given time, then one would find a continuity correlation of approximately .40 between the observed attitude at time 1 and at time 2 ($.65 \times .95 \times .65 = .40$). In addition, one would find a correlation of .38 over four years ($.65 \times .90 \times .65 = .38$), a figure only slightly lower than the two-year correlation of .40. In empirical research, a difference of this magnitude might not be detected, leaving the impression that there was literally no decay over time. Over long periods of time, the decline in correlations would become more pronounced, of course. Over an eight-year period, stability in the latent attitude would fall to $r = .81$, resulting in an observed attitudinal correlation of .34, which would be less likely to pass unnoticed. But over a period of four years, the observed results from the Latent Attitude model would closely approximate those from the Black and White model in Figure 1.

In other respects, however, the two models have profoundly different implications, which can be tested empirically. Given a two-year continuity correlation of .39, the Black and White model implies that the overwhelming majority of respondents have no real preferences and are giving random answers. The Latent Attitude model postulates that all or most respondents have real preferences. At any given time, some may be giving random answers (this is one possible interpretation of why there is an imperfect correlation between latent and observed attitudes), but if so, it holds true of only a minority of the sample. The Black and White model postulates that the public consists of two radically different types of respondents: Blacks, characterized by total flux, and Whites, characterized by total stability, whereas the Latent Attitude model implies that the public is characterized by various shades of gray. Finally, the Latent Attitude model implies that one may find certain patterns of aggregate stability in the attitudes of mass publics that could not exist if the Black and White model applied. In the next section I present extensive evidence of such stability.

The Black and White model is more parsimonious than the Latent Attitude model in one respect; it deals only with directly observed attitudes, whereas the Latent Attitude model postulates the existence of a second level of attitudes which is not observed directly. On the other hand, the

Black and White model is less parsimonious in that it postulates the existence of two radically different types of respondents, whereas the Latent Attitude model only requires one. Thus far, no one has been able to isolate the Blacks from the Whites, and I suspect that the task cannot be accomplished. However, the Latent Attitude model raises the question, "Why is there an imperfect correlation between underlying preferences and observed responses?"

One possible answer is because the questions are too vague or confusingly worded; better-written questions would produce a 1.00 correlation between the respondents' real preferences and the observed responses, resulting in much higher levels of stability over time and much greater ideological coherence. This possibility was emphasized in some of the early criticism of the Black and White model. But although this explanation may apply in certain cases, its general applicability has become almost impossible to maintain. During the past few decades hundreds of investigators in dozens of countries have attempted to develop and administer clear and meaningful political questions in public opinion surveys. The results have been dismayingly uniform. Political party identification often shows high stability, but apart from methodological artifacts, one almost never finds high levels of constraint or stability in response to questions about policy preferences among the general public. On the other hand, one does find relatively stable and coherent policy attitudes among political elites, even in response to the same questions that produced low stability and constraint among the general public (Converse & Pierce, n.d.; Inglehart, 1984; Putnam, Leonardi, & Nanetti, 1979). Thus the problem does not seem to lie primarily in the questions themselves, but rather is largely one of motivation and time. Even if people have preferences that are relevant to a specific sociopolitical question, it may take time and effort to articulate them. In the typical survey situation, a stranger appears at the respondent's door and asks him or her to respond to as many as 300 or 400 questions with a promise that the interview will be over within a reasonably short time. Most members of the public are cooperative under these circumstances. They give answers and sometimes very thoughtful ones, but the process of working out the cognitive implications of one's basic preferences may involve a lot of work. When faced with a large number of questions on which he or she has not previously articulated a position, the respondent may be under considerable time pressure. One is most likely to obtain stable responses that reflect basic preferences in connection with those topics that the respondents have frequently discussed and about which they

have already made the necessary inferences before the interview.

It is probably for this reason that one obtains substantially higher stability coefficients for political party identification than for virtually any other political variable. Throughout one's life, one is repeatedly called on to express overt support for one party rather than another through explicit actions such as voting and in political discussions and other forms of political action. One's religious affiliation is one of the few variables that shows a comparable degree of stability, probably for similar reasons. A majority of the American public report that they attend church at least once a month. With both political and religious orientations, the likelihood is relatively high that the respondent has already worked out the necessary cognitive connections before the survey interview takes place.

The key point is that it may take a good deal of effort to put one's gut feelings into words. Zajonc (1980) argues that affective and cognitive orientations are distinct systems that influence each other but have a large degree of autonomy; furthermore, reversing the tendency to view one's basic preferences as the logical consequence of all one's relevant cognitions, he presents evidence that affect may precede cognition (Zajonc, 1980; Zajonc & Markus, in press; Zajonc, Pietromonario, & Bargh, 1982). In other words, one does not necessarily start with articulate, rational considerations and then derive general preferences from them; it is an interactive process in which feelings sometimes come first. Reasoning along similar lines, I suggest that even the apparently "random" respondents in panel surveys often have underlying affective orientations that are only loosely linked with their cognitions about specific public policies, but are logically relevant to them. Although affective orientations seem to arise quickly and effortlessly, the process of working out their logical implications for each of the innumerable policy questions that face a society would require a great deal of time and effort. Unless they are involved in daily discussion of policy questions, most people have not made all of the relevant inferences. When asked their opinions in a public opinion survey situation, they do not always stop and take the time required to derive the policy implications of their underlying preferences. Instead they may give superficial answers that are not necessarily consistent with their basic value preferences. Consequently, random answering is most likely to occur when the respondent does not start the interview with a prefabricated cognitive-affective linkage relating to the given topic.

Thus, although I reject the Black and White model, I am in partial agreement with Converse:

random answering does play an important role in survey research. But our interpretations differ in several respects. Mine implies that: 1) Random answering is much less widespread than the Black and White model suggests, and evidence presented below indicates that at any given time it is limited at most to 20 to 30% of the sample; 2) Random answering is not concentrated among a distinct group of respondents who have no relevant preferences and therefore always give random answers on a given topic, but is scattered throughout the sample; 3) Random answering is largely a question of time and motivation. Consequently, under favorable circumstances the percentage of the public giving constrained and stable answers can become relatively large; conversely, with low incentive and high time pressure, it increases; and 4) If relevant preferences exist and the problem is mainly one of measurement error, then multi-item indicators should enable us to tap these preferences more accurately and with greater stability and constraint than their components.

During the past decade, the development of increasingly refined techniques has led to cumulative advances in measuring latent variables and estimating the impact of measurement error (Achen, 1975; Erikson, 1978; Jackson, 1983; Judd & Milburn, 1980; Pierce & Rose, 1974). The results provide increasingly strong support for a latent attitude interpretation. In the latest and most advanced of these investigations, Jackson (1983) has reanalyzed the data examined by Converse using the LISREL structural equation approach.

He finds: 1) a substantial amount of measurement error, although whether this is the result of nonattitudes among respondents or poor questionnaire design is not ascertained; 2) a good deal of constraint in the form of a common policy orientation underlying the issue questions included in the panel; and 3) a relatively high level of stability: the common policy orientation shows a continuity correlation of .66 across the four-year period from 1956 to 1960.

Jackson's analysis explains the observed phenomena in an elegant and persuasive fashion. Nevertheless, there is still some room for doubt about this type of explanation, because to some extent, one's results depend on how one constructs the model. Although Jackson's results point in the same general direction, they are significantly different from those obtained by Achen (1975) or Erikson (1978) or Judd and Milburn (1980). The raw data are straightforward, and they show low stability. Evidence of underlying stability emerges only after the data have been subjected to some relatively complex processing. As Converse put it in his response to

Judd's and Milburn's LISREL analysis:

To my eye, the authors obliterate these signs of confusion analytically and then, turn around to proclaim proudly that no such confusion exists, at least not "really," for example in the estimates from the structural model. I am not unaware of what this kind of "real" means. At the same time, I cannot forget how the data looked before the authors began to process them, and I am convinced that those raw data say something about the reality out there, too. (Converse, 1980, p. 646)

Is the stability that Jackson and others detect produced by statistical sleight-of-hand, or does it reflect stability in the real world of a kind that sometimes manifests itself directly? In the following section, I present a massive body of empirical evidence that strongly supports the latter interpretation. This evidence indicates that the belief systems of mass publics are underpinned by remarkably stable components that seem to persist not merely over periods of two to four years, but over decades.

This evidence is of two complementary kinds. The first kind is panel surveys carried out over various intervals, ranging from two months to seven years. In each of these surveys the respondents gave high or low priority to various societal goals; the results show continuity correlations that are rather modest over short periods, but decay very slowly over long periods of time, in keeping with the Latent Attitude model. The second kind of evidence is cohort analysis of these same goals, as ranked by successive cross sections of western publics at numerous time points across the period from 1970 through 1984. The results display a remarkable component of stability, in the form of substantial differences between birth cohorts which have persisted throughout the tumultuous economic ups and downs that characterized this era. And—of crucial significance here—this stability is structured in a way that is incompatible with a Black and White model; it can only be explained by a Latent Attitude model, because the respective birth cohorts maintain very different positions throughout the 14-year period. Among the oldest cohort, there is a huge preponderance of materialist over postmaterialist responses; among the youngest cohort, the two types are almost equally numerous. These distinctive skews could not result if more than a small minority were answering at random, and they cannot be attributed to methods effects because both young and old were asked the same questions in the same format. Such a pattern could only result from relatively stable, long-term attitudinal predispositions within all or most of the respondents themselves.

Examining the panel survey response to these items might lead one to underestimate grossly this underlying stability, however, because the turnover is as high as that found with the items used to illustrate the Black and White model. There is no alternative but to turn to a Latent Attitude model. When I do so, LISREL analysis yields results similar to those Jackson and others have obtained with data from the 1956-1960 panel. Although there is substantial measurement error at any given time, most respondents have underlying preferences that remain stable throughout the seven-year period of our longest panel.

The problem is not nonattitudes so much as measurement error. Accordingly, using better measurement techniques, one finds much greater stability. This point can be made more directly, even without going into the complexities of LISREL. In addition to the simple and widely used four-item battery designed to measure materialist/postmaterialist value priorities, a broader-based 12-item battery was also developed in 1973 and has been used in several surveys since that time. As one would expect, 12 items enable us to tap the underlying attitudinal dimension more accurately than is possible with four items. Consequently the 12-item index consistently shows greater stability over time and greater constraint with other attitudes than does the 4-item index.

Similarly, when we move from the public-opinion-survey situation to a classroom setting in which the context tends to stimulate higher levels of interest in the task of articulating one's values and provides a more leisurely setting in which to do so, there are substantially higher levels of stability—even among respondents with relatively modest educational levels.

Let us now examine the empirical evidence on each of these points, starting with a cohort analysis based on cross-sectional survey data extending over 14 years. I will compare these results with findings from a two-wave panel survey carried out in the United States in 1974 and 1981 and (more briefly) with results from other panel surveys.

Cohort Analysis of the Values of West European Publics, 1970-1984

The theoretical framework underlying my cohort analysis implies that there has been a gradual intergenerational change in the relative emphasis placed on certain basic goals—a shift from materialist toward postmaterialist values (Barnes, Kaase et al., 1979; Inglehart, 1971, 1977, 1981). If intergenerational change can be demonstrated, it reflects the persisting effects of distinctive cohort experiences that may have taken place as much as four or five decades ago, and will

constitute strong evidence that mass publics have enduring sociopolitical preferences.

The materialist/postmaterialist thesis is based on two key hypotheses: 1) a *scarcity hypothesis*, that an individual's priorities reflect one's socioeconomic environment, so that one places the greatest subjective value on those things that are in relatively short supply, and 2) a *socialization hypothesis*, that to a large extent, one's basic values reflect the conditions that prevailed during one's pre-adult years.

The scarcity hypothesis implies short-term changes, or period effects; periods of prosperity lead to increased postmaterialism, and periods of scarcity lead to materialism. The socialization hypothesis implies that long-term cohort effects *also* exist; the values of a given generation tend to reflect the conditions prevailing during its pre-adult years. The theory implies nothing about aging or life-cycle effects.

Taken together, the two basic hypotheses imply that the process of value change is characterized by period effects (reflecting short-term fluctuations in the socioeconomic environment) superimposed on long-term cohort effects (reflecting the conditions prevailing during a given age group's formative years).

An empirical test of this theory requires that one distinguish among period effects, cohort effects, and life-cycle effects. This is not an easy task. The three types of effects are linearly depen-

dent on one another and cannot be statistically distinguished from each other if the only information one has is each respondent's year of birth and his or her age at the time of each survey. In order to distinguish among the three effects, one must have theoretical grounds for ruling out or controlling for at least one of the three.

The original 4-item materialist/postmaterialist values battery has now been administered to representative national samples of the populations of Britain, France, West Germany, Italy, Belgium, and the Netherlands in 11 different years (from 1970 to 1984) in surveys sponsored by the Commission of the European Communities. This massive data base is now reaching the point where it provides measurements at enough time points to trace the rise and fall of period effects and to correlate them with their underlying causes. In order to maximize reliability, my cohort analysis is based on the pooled samples from all six nations. These pooled results, providing an average of approximately 2,000 cases per age cohort in each year, enable one to follow the value priorities of West European publics across a 14-year period that began with high prosperity, but later was characterized by two recessions and runaway inflation.

Before I examine the cohort analysis, let us ask: exactly what kind of pattern does each of the three types of effects imply? Figures 2-4 depict three possible patterns. Figure 2 depicts the pat-

Figure 2. Cohort Effects Only

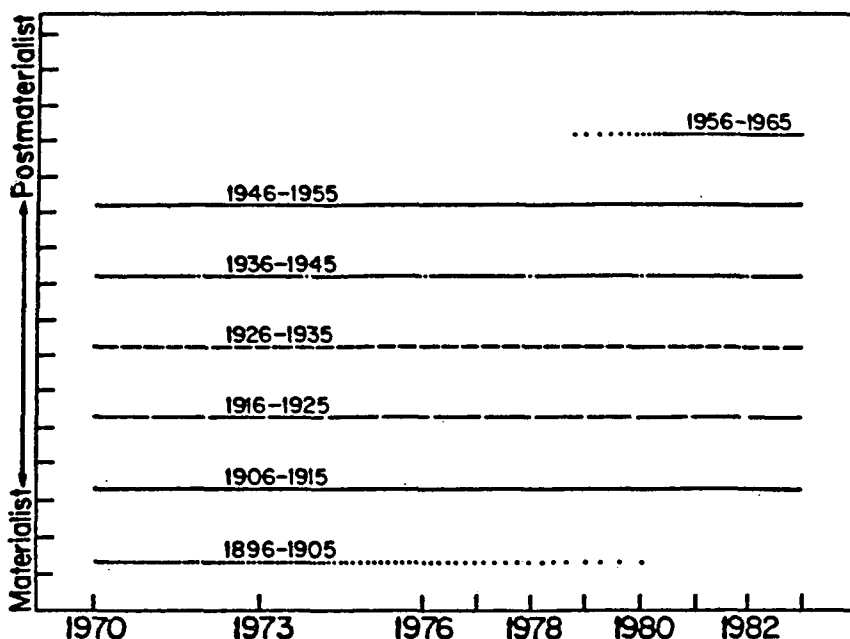


Figure 3. Cohort Effects Plus Period Effects

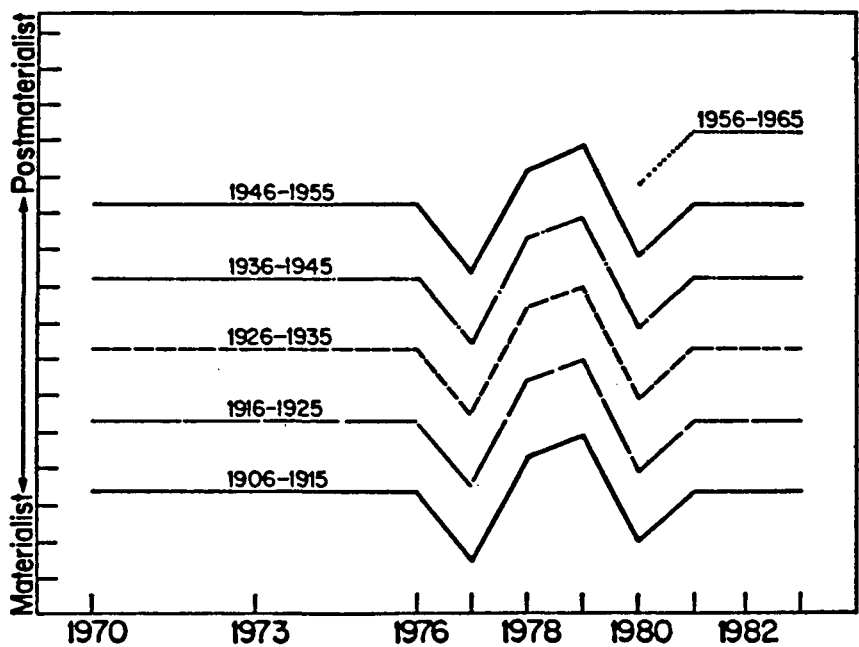
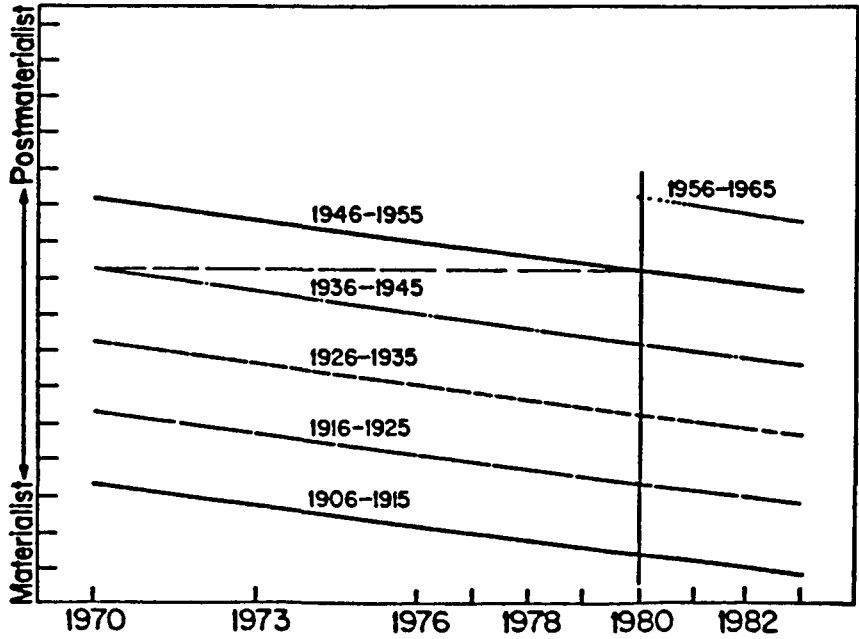


Figure 4. Life Cycle Effects Only



tern of value differences one would find if early socialization were the only influence on adult values, so that cohort effects, and only cohort effects, were present.

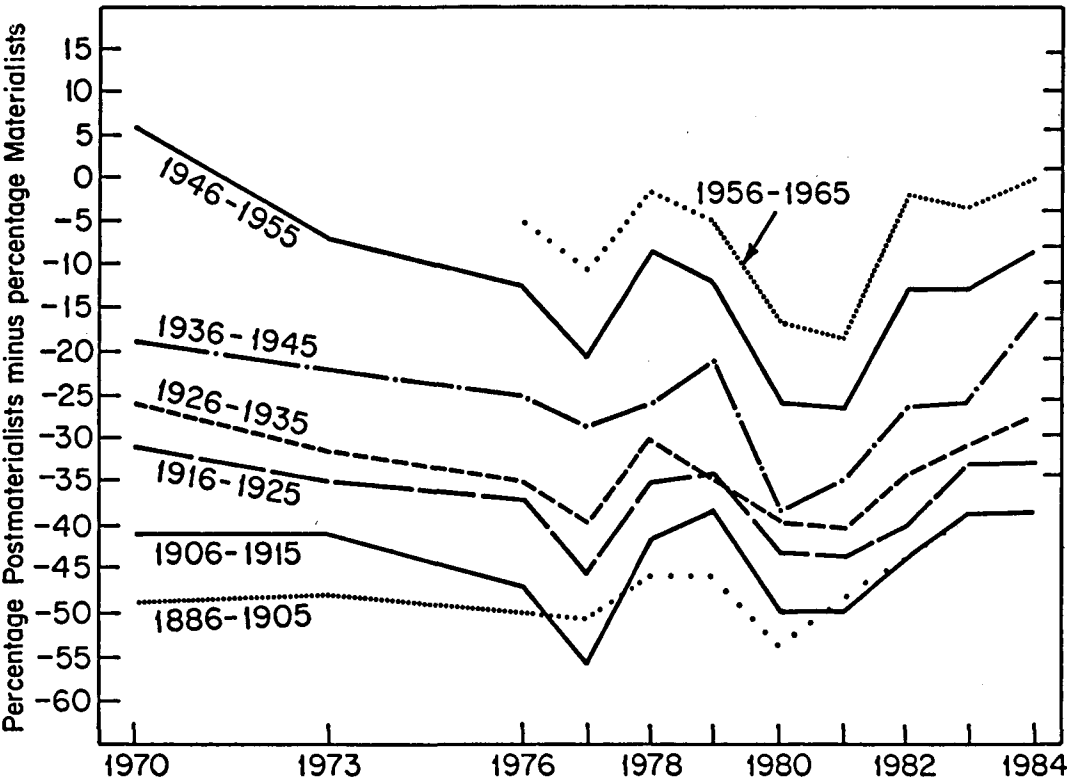
In this ideal-type model, the younger birth cohorts are less materialist than all of the older cohorts at all points in time. Because no period effects are present, each cohort's values remain absolutely unchanged, regardless of any changes in the socioeconomic environment. But because of population replacement, the values prevailing in a given society do change over time. During the 14-year period depicted here, approximately 70% of the 1886-1905 birth cohort died; they were replaced in the adult population by a much more numerous cohort born from 1956 to 1965, a group too young to be included in our samples in 1970, but which constituted a major part of the adult public by 1984. The fact that this youngest and much more postmaterialist cohort replaces the oldest, most materialist one would tend to produce a net shift toward postmaterialism.

But my theory is based on a scarcity hypothesis

as well as a socialization hypothesis: one can expect period effects as well as cohort effects. Figure 3 depicts a pattern of period effects superimposed on stable cohort differences. Although one can observe substantial fluctuations in response to short-term forces in this figure, the cohort effects are fully as strong as those in Figure 2. Consequently, in both cases the process of intergenerational population replacement tends to produce a gradual shift toward postmaterialist values.

Figure 4 depicts a model in which age-group differences exist, but result entirely from life-cycle effects. As a given birth cohort ages it comes to resemble the next older cohort, so that after 10 years have elapsed, a given cohort has shifted to the position held by the cohort that is 10 years older. Although each cohort does change over time, the values of the society as a whole do not change, because population replacement is offset by life-cycle effects. Empirically, life-cycle effects can be distinguished from cohort effects because (aside from short-term fluctuations) in the former, each cohort manifests a downward long-

Figure 5. Value Priorities of Six West European Publics, by Birth Cohort, 1970-1984



Based on representative national samples of the publics of France, Great Britain, West Germany, Italy, The Netherlands and Belgium, interviewed in European Community surveys of 1970 and 1973, plus Euro-Barometers 6 through 21 (total N = 141,133).

term trend, whereas in the latter, each cohort remains horizontal, when plotted as in Figures 2-4. Although my theoretical framework does not exclude the possibility of life-cycle effects, it does not predict them; however, it does predict that there will be substantial and durable cohort effects.

Do we find them? To answer this question, let us turn now to a cohort analysis of the Euro-Barometer data gathered in six West European nations from 1970 through 1984. When two surveys were carried out in a single year, I combine them to maximize sample size. Figure 5 shows the results of the combined six-nation sample. Separate analyses for each country are shown in Inglehart (1985); the basic pattern is similar cross nationally.

In Figure 5, each cohort's position at a given time is calculated by subtracting the percentage of materialists in that cohort from the percentage of postmaterialists. Thus, the zero point on the vertical axis reflects a situation in which the two groups are equally numerous (which is approximately where the cohort born in 1946-1955 was located in 1970). An index of -45 would result if, for example, 50% of a given cohort were materialists and only 5% were postmaterialists (with the rest being mixed types): the oldest cohort was located slightly below this point in 1970.

Figure 5 summarizes the results from an immense amount of survey research based on more than 140,000 interviews. It demonstrates rather conclusively that the age-group differences observed in 1970 reflect long-term intergenerational differences rather than life-cycle effects.

Each cohort retains its relative position with striking consistency throughout the 14-year period. The 1946-1955 cohort is less materialistic and more postmaterialistic than any of the older cohorts at every point in time. The only cohort that is even less materialistic is another postwar cohort, born from 1956 to 1965—a group that was too young to be interviewed in 1970, but which becomes an increasingly important part of my sample from 1976 on. As Figure 5 demonstrates, each of the older cohorts proves to be more materialist than all of the younger ones at every time point, with only a few minor anomalies. The intercohort value differences are extremely stable.

Moreover, there is no indication at all that each cohort becomes more materialist as it ages, as would be the case if these differences reflected life-cycle effects. At the end of the 14-year period, virtually all of the cohorts were fully as postmaterialistic as they were in 1970. Indeed, there is something of an upward tendency; most of the cohorts are less materialist in 1984 than they were in 1970. There are also some significant short-term fluctuations; each cohort shows a brief

downward swing in 1977 and again in 1980-1981. These fluctuations reflect period effects that result largely from the impact of inflation, as I demonstrate below. But by 1984, inflation had subsided almost to the 1970 level. With period effects held constant, there is no sign at all of the gradual conversion to materialism that would be present if a life-cycle interpretation were applicable. The pattern reflects intergenerational value change.

These stable intergenerational value differences imply that, other things being equal, we will witness a long-term trend toward postmaterialist values as one generation replaces another. A good deal of intergenerational population replacement had already taken place from 1970 to 1984. During these years, approximately 70% of the 1886-1905 birth cohort died. They were replaced by a much more numerous cohort born after 1955. In 1970 the postwar generation constituted only 20% of the sampling universe (those citizens over 15 years of age); in 1984 it constituted fully 46%.

There has been a corresponding shift in the distribution of value types. In 1970, materialists outnumbered postmaterialists in these six nations by a ratio of 3.4 to 1. In 1984 this ratio had fallen to little more than 2 to 1. Contrary to predictions that postmaterialism would disappear as a result of the economic crisis, the underlying process of intergenerational change continued to function throughout the period, even though its effect was sometimes masked by negative period effects. When short-term forces returned to normal, the results were manifest: a substantial net shift toward postmaterialism had taken place—most of it the result of intergenerational population replacement (see Abramson & Inglehart, 1984).

Perhaps the most striking feature of Figure 5 is the persistence of stable differences between the value priorities of the respective cohorts across a period of 14 years. In all of the years for which data are available, those born in 1956-1965 constitute the most postmaterialist cohort. At all 11 time points, the 1946-1955 cohort ranks second, above all of the older cohorts. At all 11 time points, the 1936-1945 cohort ranks third; at 10 of the 11 time points, the 1926-1935 cohort ranks fourth; at 10 of the 11 time points, the 1916-1925 cohort ranks fifth; and the 1886-1905 cohort ranks last or second to last in all 11 years. Of 75 points depicted in Figure 5, there are only four anomalies, and all of these are minor ones which involve an overlap between two immediately adjacent cohorts and could be caused by samples that deviate by only a few percentage points from the actual values. The pattern is about as close to perfection as one could hope for: the relative positions of the respective cohorts are extremely stable.

The data show a virtually perfect fit with theoretical expectations. As predicted, there are substantial differences between the values of different cohorts and, as predicted, the younger cohorts are consistently less materialist than the older ones. Moreover, as predicted, these differences seem to reflect cohort effects: there is no indication of the long-term downward trend that would be present if one were dealing with life-cycle effects. A given cohort does not become more materialist as it ages. The overall tendency from 1970 to 1984 is horizontal. The respective cohorts are generally at least as postmaterialist in 1984 as they were in 1970, and if there is any net trend at all, it is upward, toward increasing postmaterialism.

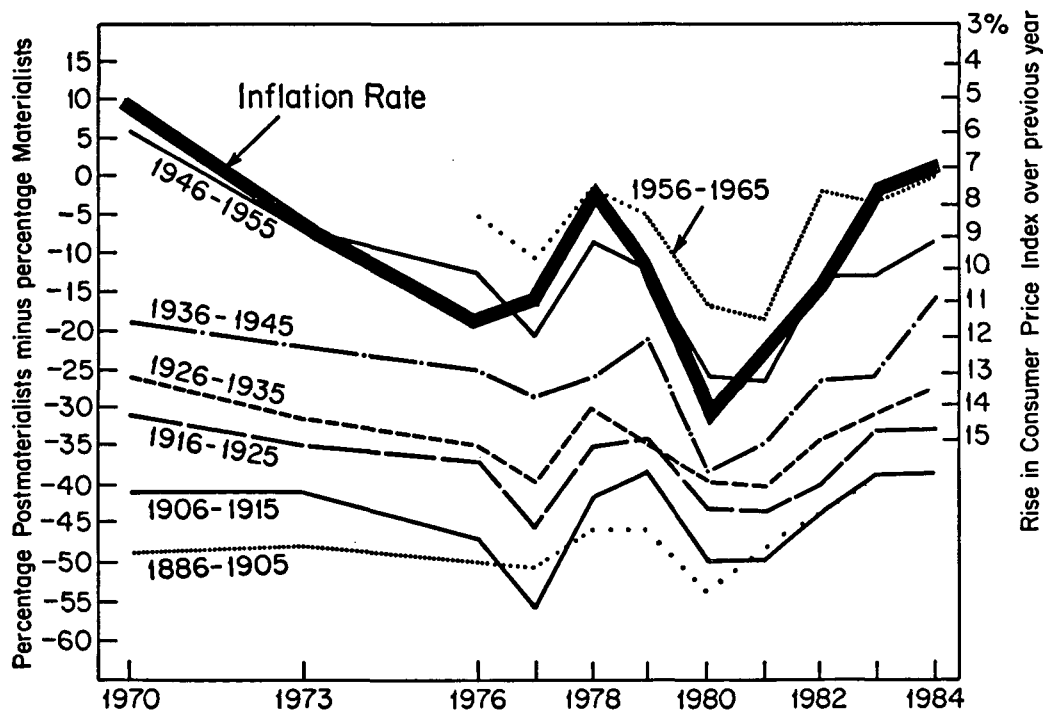
The cohort differences are unmistakable, but there is clear evidence of significant period effects as well, and my interpretation of the cohort analysis will not be complete until I have accounted for them. It is not difficult to do so. When I formulated the battery of items first used to measure materialist-postmaterialist value priorities in 1970, I hypothesized that one's sensitivity to inflation would be a good indicator of materialist priorities. Consequently, one of the two materialist options in the four-item battery

used here is "fighting rising prices." My expectation was that different age cohorts, whose socialization had been shaped by different historical experiences, would respond differently to this item, with the older cohorts more apt to give it a high priority.

When I formulated these items in 1969, I did not anticipate the explosive worldwide inflation that would later take place, following the OPEC oil price shocks of late 1973 and late 1979, respectively. But the logical implications are clear: one would expect such inflation to enhance the chances that a high priority would be given to "fighting rising prices." The theoretically obvious interpretation of the period effects found in Figure 5, then, is that they reflect the two waves of inflation that impinged on Western Europe so dramatically in the mid-1970s and again at the start of the 1980s. Is this interpretation confirmed empirically?

Figure 6, which shows the answer, is identical to Figure 5 except that it shows the mean inflation rate for the six nations from 1970 through 1983. The inflation rate is indicated by the rise in the Consumer Price Index during the year of the survey; high inflation rates are reflected in a downward movement of the inflation line, so that

Figure 6. Value Priorities and Inflation Rates in Six West European Countries, 1970-1984



Both inflation rates and values indices are weighted according to population of given nation.

there is the same polarity for the two sets of variables. In those years for which no survey data are available, the inflation rate is not plotted either.

Again, the fit between data and theory is remarkably good, so good that it is immediately apparent even from simply scanning Figure 6. Each of the two dips toward increased materialism reflects a rise in inflation, and the upward movements of 1978-1979 and 1982-1983-1984 reflect the abatement of inflation, with roughly a one-year lag.

One's impression that the period effects result largely from changes in the inflation rate is confirmed by multiple regression analysis of the data in Figure 6 (see Inglehart, 1985). In this analysis, the dependent variable is the value index for a given cohort in a given year. Inflation is operationalized as the mean change in the Consumer Price Index in these six nations during the year preceding the survey. Life-cycle effects are operationalized as the mean age of a given cohort in a given year; if life-cycle effects are present, each cohort should become more materialistic as it ages. On the other hand, cohort effects theoretically remain constant over time within each birth cohort, so they are operationalized as constants. The oldest cohort is coded as 1 at each time point, the next oldest as 2, and so on, with the youngest cohort coded as 6 at each time point.

Operationalizing the variables in this simple but straightforward way resolves the problem of underidentification that otherwise besets cohort analysis. In the multiple regression analysis based on these variables, the combination of cohort effects plus period effects explains fully 87% of the variance in value priorities, with cohort effects the stronger of the two predictors (the respective regression coefficients are .97 and $-.32$). Life-cycle effects explain literally no additional variance. One could probably explain a somewhat larger percentage of the variance with a more complex regression model that took account of such other variables as unemployment rates or indexes of industrial production, which might also play a role in explaining the short-term fluctuations. But the combination of cohort effects and inflation rates explains such a high proportion of the variance that it is hardly necessary to develop a more elaborate model. The central question here is whether or not intergenerational differences are a major factor, and the evidence clearly indicates that they are.

These findings have extremely important substantive implications (Inglehart, 1981, 1984) which I will not explore here. What is important for present purposes is that these findings are incompatible with the implications of the Black and White model. Although the stability correlations

of the values index are no higher than that of the item used in Converse's classic study, the empirical pattern observed here could not possibly result from a situation in which a majority of the respondents were giving random answers. Literally random answering would result in equiprobable response distributions, producing a 50:50 split when there are two alternatives. If as much as 80% of the public were giving random answers, they would show a 40:40 split, and even if all of the remaining 20% chose the same option (which is highly unlikely), the most extreme skew possible would be a 60:40 split.

In the present case, pure random answering would produce equal numbers of materialists and postmaterialists, and indeed among our youngest cohorts, postmaterialists are almost as numerous as materialists. But among the cohort born before 1906, the situation is drastically different. In a number of countries, well over 50% of the sample falls into the pure materialist category, whereas only about 4% give postmaterialist responses. By purely random processes, 16.7% would fall into the latter category: the results indicate that no more than 25% of the cohort could be giving random answers, even if one assumes that none of the observed postmaterialist responses among this oldest cohort reflects genuine preferences within the respondent.

In itself, this point is not entirely new. A decade ago, Converse (1974) and Pierce and Rose (1974) had agreed that some observed response patterns could not be attributed to equiprobable random answering (such as would result from rolling unloaded dice); the distributions sometimes displayed skews that could only result from random answering that was not equiprobable (such as would result from rolling loaded dice).

But a crucial question could not be resolved in this debate and remained to be answered: What made the dice loaded?

Pierce and Rose attributed the observed deviations from equiprobable random answering to unmeasured attitudinal predispositions: the low stability correlations did not result from non-attitudes but from imperfect measurement of them. Converse, on the other hand, attributed the skew to methods effects; if the deviations from equiprobable distributions resulted from response set or other artifacts of questionnaire design rather than from underlying preferences within the respondent, the nonattitudes thesis could still apply.

On this point, my cohort analysis provides decisive evidence. The older cohorts display skews that deviate enormously from an equiprobable distribution, and it is impossible to attribute these deviations from pure randomness to any sort of methods effect because both young and old

cohorts were asked precisely the same questions, in the same format, at the same time, and they show drastically different distributions. The differences in responses reflect differences in the respondents, not something in the measuring instrument.

The current evidence is incompatible with Converse's interpretation. The findings *must* result from unmeasured attitudinal predispositions, and the differences in the underlying attitudes of old and young are remarkably enduring. Already, there are direct measurements indicating that these attitudes have persisted throughout a period of 14 years. Further, these findings fit almost perfectly into a theoretical framework which implies that the generational differences can be traced to the persisting impact of formative experiences that took place as much as 50 years ago (Inglehart, 1977). In short, the evidence indicates that underlying the relative fluidity of given attitudinal indicators, the sociopolitical orientations of western publics have components of remarkably great stability.

This stability is by no means limited to the materialist/postmaterialist dimension. Probably the most influential and widely recognized effort to analyze human values with the use of survey methodology has been the work of Rokeach (1968, 1973). The Rokeach terminal values survey was included in both waves of the American panel survey; it enables us to compare these findings

with results from a battery designed to measure the entire spectrum of human values.

Although my items refer to sociopolitical priorities whereas Rokeach's items tend to have more personal referents, the stability of the Rokeach items varies in the same fashion as that of the Inglehart items. Stability coefficients for the 18 items in the Rokeach battery, together with the materialist/postmaterialist index, are shown in Table 1.

As Table 1 demonstrates, materialist/post-materialist values show a substantially lower stability than the priority one gives to "salvation." Fully 25% of the American panel ranked "salvation" first among the 18 terminal values in 1974, and of those who did so, 69% ranked it first among the 18 items in 1981 as well. Conversely, a relatively large share of the sample (almost 15%) ranked "salvation" last in 1974, and 54% of those who did so ranked it last again in 1981. If one believes in religious salvation, it is the most important thing in the world—one is apt to think about it often, pray, attend church regularly, and consistently give it a very high priority. On the other hand, if one does not believe, it seems altogether useless. Thus, this basic component of one's religious outlook is by far the most stable of Rokeach's 18 terminal values.

However, the other Rokeach values items all have stability coefficients in the same range as my

Table 1. Stability of Values from 1974 to 1981 among American Public

	<i>r</i> ^a
Salvation (saved, eternal life)	.68
A comfortable life (a prosperous life)	.44
Mature love (sexual and spiritual intimacy)	.41
National security (protection from attack)	.41
Materialist/postmaterialist values	.39
A world of beauty (beauty of nature and the arts)	.39
Inner harmony (freedom from inner conflict)	.38
Self-respect (self-esteem)	.36
Equality (brotherhood, equal opportunity for all)	.36
Family security (taking care of loved ones)	.35
An exciting life (a stimulating, active life)	.34
Pleasure (an enjoyable, leisurely life)	.34
Social recognition (respect, admiration)	.32
Happiness (contentment)	.30
Freedom (independence, free choice)	.29
True friendship (close companionship)	.29
A world at peace (free of war and conflict)	.28
Wisdom (a mature understanding of life)	.26
A sense of accomplishment (lasting accomplishment)	.26

Source: Two-wave panel survey of American public carried out in summer 1974 and summer 1981 by Survey Research Center, University of Michigan. Panel *N* = 933. Fieldwork and sampling are described in *Political Action* codebook.

^aProduct moment correlations between 1974 and 1981 rankings.

materialist/postmaterialist values index. Indeed, the latter ranks higher than 14 of the 18 terminal values. This does not mean that the Inglehart battery is "better" than most items in the Rokeach battery. On the contrary, it is clear that my values index shows greater stability over time than most of the Rokeach terminal values simply because it is based on multiple indicators and therefore measures the underlying dimension more accurately than any single item does. The stability across time of our individual items is no greater than that found for most of Rokeach's items (and, of course, much lower than that of "salvation"). The problem is not that stable values do not exist, but that it is difficult to measure them. As measurement techniques improve, stability rises.

Deciding one's basic priorities in life is a demanding task for which the only reward is the intrinsic interest of the task itself. Among those who are not interested (at least not at the time of the given survey), some respondents apparently avoid a difficult and unfamiliar job by giving hasty and more or less "random" answers. This process seems to apply to the Rokeach battery as well as to the Inglehart battery. But despite the relatively modest individual-level stability found with most of the Rokeach items, I believe that they too tap basic and deep-rooted values among the American public. If so, one should find con-

siderable stability in the aggregate rankings of these variables.

One can test this expectation against time series data extending over a 13-year period, because the Rokeach terminal values survey has been administered to representative national samples of the American public in 1968, 1971, 1974, and 1981. The overall rankings obtained in each year are shown in Table 2.

The stability we observe is absolutely phenomenal. The six items that were ranked highest in 1981 are identical with the six items ranked highest in 1968, and—amazing as it may seem in view of the social upheavals that took place in this era—not one of the six items varies by more than one rank from its 1981 position in any of the three previous surveys extending across 13 years. The stability of the six lowest-ranking items is almost equally impressive: the six lowest items in 1981 are identical with those in 1968, and most of them do not vary by more than one rank from their 1981 positions in any of the three previous surveys. The public's evaluation of an "exciting life" is relatively volatile: it rises from 18th place in 1968 to 15th place in 1981. By most standards this would be considered an extremely small change. In the present context, it is an exceptionally large one. Although the middle six items show greater volatility than the top or bottom six, they remain within the middle zone almost without exception.

Table 2. Ranking of 18 Personal Values by the American Public, 1968-1981

	1968	1971	1974	1981
A world at peace (free of war and conflict)	1	1	2	2
Family security (taking care of loved ones)	2	2	1	1
Freedom (independence, free choice)	3	3	3	3
Happiness (contentedness)	4	6	5	5
Self-respect (self-esteem)	5	5	4	4
Wisdom (a mature understanding of life)	6	7	6	6
Equality (brotherhood, equal opportunity for all)	7	4	12	12
Salvation (saved, eternal life)	8	9	10	9
A comfortable life (a prosperous life)	9	13	8	8
A sense of accomplishment (lasting contribution)	10	11	7	7
True friendship (close companionship)	11	10	9	10
National security (protection from attack)	12	8	13	11
Inner harmony (freedom from inner conflict)	13	12	11	13
Mature love (sexual and spiritual intimacy)	14	14	14	14
A world of beauty (beauty of nature and the arts)	15	15	15	16
Social recognition (respect, admiration)	16	17	18	18
Pleasure (an enjoyable, leisurely life)	17	16	16	17
An exciting life (a stimulating, active life)	18	18	17	15

Source: Representative national samples interviewed by the National Opinion Research Center, University of Chicago, in 1968 and 1971 (see Rokeach, 1974), and by the Survey Research Center, University of Michigan, in 1974 and 1981.

The overall stability of the Rokeach rankings is extremely impressive. To attribute this stability to sheer chance would strain one's credulity beyond the breaking point. Despite their modest reliability coefficients, the conclusion is inescapable that the Rokeach items tap orientations that are very stably anchored among the public.

Here again, as with the materialist/postmaterialist values items, the distributions are incompatible with the assumption that most of the public is giving random answers. By random processes, each of the 18 goals would be given top priority (or lowest priority) by about 5.5% of the sample. In fact, both "a world at peace" and "family security" are chosen consistently for top priority by about four times as many people as would do so at random. At the other end of the scale, the deviations from a random model are even more extreme: the two combined are ranked last by less than 1%. There is no way in which this could occur if a majority of the sample were answering at random. If only 10% of the sample were answering randomly, these goals would be ranked last by larger numbers than those we actually observe. Again, it would be extremely difficult to attribute these deviations from equiprobability to methods effects. The respondent chose each value from a pool of items printed on adhesive labels and gave it a unique ranking. The usual forms of response set—the tendency to agree, or to disagree, or to give the same answer as one gave to the last question—do not apply here (as was also true of the materialist/postmaterialist battery). Despite the modest continuity correlations, the data manifest a degree of skewed stability that is incompatible with the assumption that nonattitudes are present among more than a small fraction of the sample.

Much of the observed individual-level instability in values apparently reflects error in measurement, rather than the absence of preferences. Because my results are generally contaminated by random answering, the responses to any one item provide an imperfect indicator of the respondent's underlying predispositions. Consequently, our multi-item values index shows greater stability over time than any of its constituent items, because it provides a more accurate measure of the underlying concept. Thus it follows that a still more accurate measure of the materialist/postmaterialist dimension should show still higher stability.

Let us try to obtain a more accurate measurement of this dimension. The materialist/postmaterialist index that was used in the panel analysis thus far simply consists of the unweighted sum of all postmaterialist items given top priority, minus the sum of all materialist items given top priority. This procedure has the virtue of simpli-

city; it is intuitively clear what a given score means. But one should be able to obtain a more accurate measure by weighting each item according to the strength of its correlation with the latent variable (as do factor scores, for example).

An even better measurement can be obtained by using LISREL analysis—a structural equation technique that not only weights each indicator, but also estimates the amount of measurement error present in given indicators and controls for correlated error terms. This is important with my values indicator, since it is based on one's mutually exclusive rankings of items from given groups. With an ipsative battery such as this, the error terms clearly are correlated.

A LISREL analysis of data from the panel study carried out in the United States produces the results shown in Figure 7. In this model, one's rankings of the various items in the materialist/postmaterialist battery are treated as indicators of the latent variable, materialist/postmaterialist values, for the given year in which the items were administered,¹ while one's values in 1974 are treated as an independent variable that accounts for one's values in 1981. This model uses a modified form of the ipsative correction proposed by Jackson and Alwin (1980).²

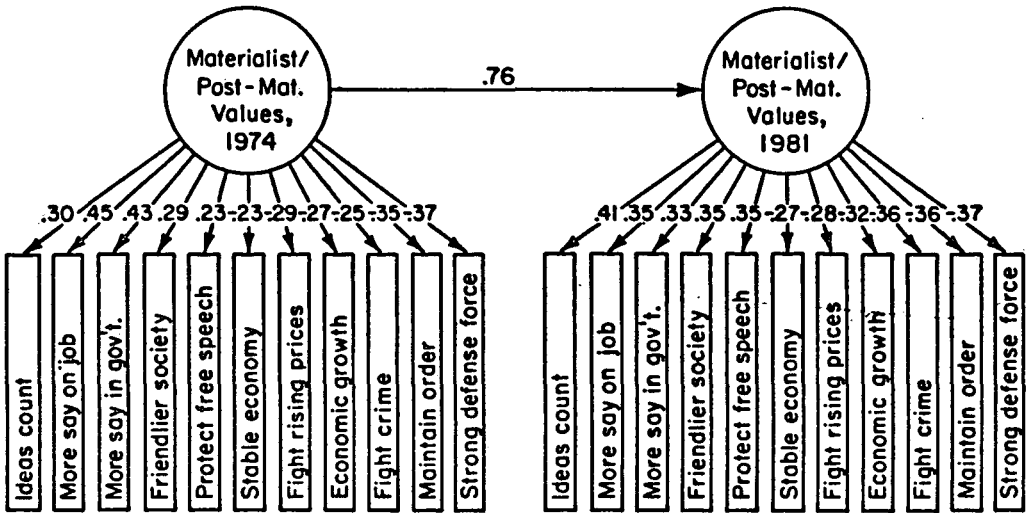
The results of this analysis confirm, once again,

¹It was necessary to omit one item from the battery presented in each year, because otherwise the rankings of any 11 items would fully determine the ranking of the remaining item: hence we have 11 indicators, even though 12 items were presented to the respondent in the interview. The item omitted from this analysis was "more beautiful cities and countryside," which was the weakest indicator of the underlying dimension.

The fact that these items are ranked largely eliminates possible contamination by response set; but it artificially depresses the factor loadings shown in Figure 7. The choice of any given item reduces the likelihood that any other item can be given high priority, which tends to make all items negatively correlated with each other. Thus, the two items with the highest negative correlation tend to define opposite poles of the first factor—but, since they are negatively correlated with both items, none of the other items can attain high loadings on this factor. Despite the seemingly low loadings, when I correct for ipsativity our LISREL model shows a satisfactory goodness of fit.

²The sampling design for the Dearborn panel survey in Figure 7 is described in Appendix A of Stockton and Wayman (1983). The University of Michigan panel was carried out in February and April 1984, with a sample of 26 students enrolled in an undergraduate political science course. Fieldwork for both the four-year panel and the seven-year panel were carried out by the Survey Research Center of the University of Michigan; for details, see ICPSR codebooks for the National Election Studies of 1972 and 1976 and the *Political Action* survey.

Figure 7. The Stability of Values among the American Public from 1974 to 1981.
LISREL Standardized Solution. Chi Square = .086, $df = 197$.



the existence of a materialist/postmaterialist dimension. All six of the indicators designed to tap materialist goals show significant negative correlations with the latent variables measured respectively in 1974 and in 1981, and all of the items designed to tap postmaterialist goals show significant positive correlations with the respective latent variables. There is a good deal of measurement error at the level of the respective indicators. Several of the individual items are relatively poor indicators of the underlying values dimension. Their correlations with the latent materialist/postmaterialist variable range from .23 to .45. Moreover, taken individually, several of these items show little stability over time (with correlations as low as .20).

But the underlying dimension has a remarkably high degree of stability across time; the panel respondents' latent value priorities in 1974 show a .76 correlation with their values in 1981, which indicates that almost 58% of the variance in our respondents' values in 1981 can be attributed to the values these same individuals held in 1974. This finding replicates results obtained by Dalton (1981), who analyzed a two-wave German panel survey. Using methods similar to mine, Dalton found a .70 correlation between materialist/postmaterialist values among German youth in 1976 and 1979. That I obtain an even higher correlation in a panel survey extending over seven years rather than three may reflect the fact that I used a somewhat different values battery and a different population from the one studied by Dalton, but it is also an indication of how surprisingly little

these correlations decline over time. The panel I have been analyzing spans seven years, an exceptionally long period. The questions used to measure materialist/postmaterialist priorities have also been asked in a number of other American panels over shorter periods of time. Let us compare the results.

Figure 8 shows the expected and empirically observed correlations between value priorities measured in panels ranging from two months to seven years. The expected values are derived by working backward from the values obtained in the seven-year panel, which shows a continuity correlation in the observed variable of .32 (as measured by the four-item index) and a continuity correlation in the latent variable of .76 (as estimated by LISREL). Given these parameters, a Latent Attitude model implies that one should find a modest but perceptible decline in the continuity correlations over various periods of time, as indicated in Figure 8. A Markov Chain model, of course, implies a much more rapid decline in the correlations for the observed variable; if the observed correlation over two months were .42, it would have declined to zero within less than a year. A Black and White model, on the other hand, implies no decline at all; if the correlation over seven years were .32, it would be .32 over two months or two decades.

The empirically observed correlations fit a Latent Attitude model much better than either of the other two models. Although they are based on different samples instead of one continuous panel, the figures are reasonable.

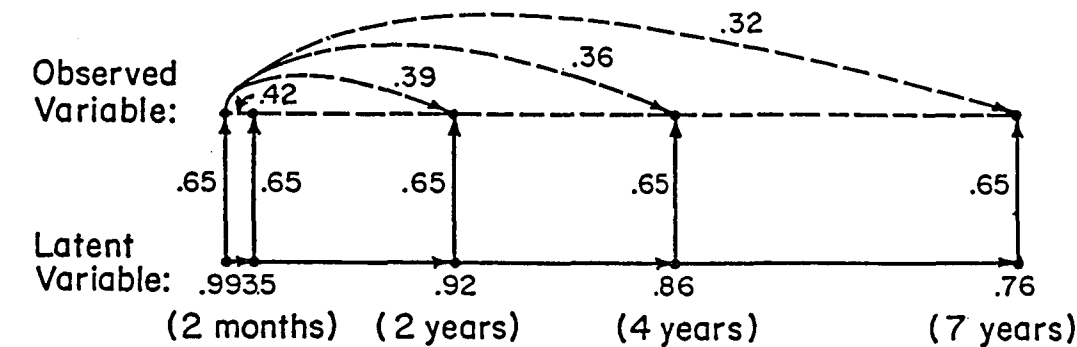
Surprisingly, however, the four-item index shows a weaker correlation over four years than over seven years! This is a result that none of the three models would have predicted. When one examines the details more closely, it seems that this deviation from logical expectations may reflect the problems of time and motivation mentioned above. The four-year panel examined here was carried out in 1972 and 1976. In 1972, these questions appeared fairly early in the interview, but in 1976, they were asked at the end of a long interview, after the respondent had already answered more than 400 questions in an interview that averaged approximately 90 minutes. By this point, respondent fatigue may have become a significant factor, making some respondents eager to get the interview finished. If so, it would tend to increase the rate of random answering. In the seven-year panel, on the other hand, these questions were

asked about seven or eight minutes into the interview in both 1974 and 1981. Here, respondent fatigue would be much less important, resulting in less error in measurement and thus the phenomenon we observe, a higher stability coefficient over seven years than that observed over four years.

Conversely, the stability coefficient observed in the two-month panel is somewhat above the expected value, possibly because we are dealing with a student sample instead of a cross-section of the population, as in the other panels. Those with more education are more likely to discuss politics than those with less education and consequently are likelier to come to the interview with an answer already articulated that reflects their underlying preferences. However, the stability coefficient might also reflect the fact that the two-month panel was carried out in a classroom rather

Figure 8. Expected and Observed Stability of Materialist/Postmaterialist Values over Various Time Intervals

1. Expected correlations for 4-item index
(assuming constant rate of decay in stability of latent variable)



2. Observed correlations:

Time interval:	Survey	Continuity Correlations:	
		4-item index	12-item index
2 months	(University of Michigan student panel, 1984)	.53	.82
2 years	(Darborn panel, Stockton & Wayman, 1974-1976)	.35	—
4 years	(National Election Study panel, 1972-76)	.28	—
7 years	(Political Action panel, 1974-81)	.32	.39

than under typical survey fieldwork conditions. In the classroom, the respondents carry out their rankings in a context that reinforces the idea that the task is important and therefore worth doing carefully. Moreover, the task is performed in a relatively leisurely setting: unlike many other survey respondents, the students are not stealing the time from other activities. Thus, a test-retest experiment carried out in a classroom situation may provide a stronger motivation to answer thoughtfully, with more leisure and fewer distractions present than is true in the usual field interview situation. These factors may account at least partially for the markedly higher stability of results obtained in the classroom situation.

The difference does not seem to result solely from the fact that one is dealing with a more highly educated population. Rokeach (1973, p. 58) reports that the median test-retest reliabilities reported for the 18 terminal values items, administered to samples of 7th and 9th grade students, were about .63 over a three-week period; for college students, they were .78 over three weeks, and (again) .78 over two months. Thus far, little research has compared the stability and constraint of the responses elicited in experimental situations with the stability and constraint obtained in survey situations. The available evidence suggests that one can obtain significantly higher levels of stability and constraint under experimental conditions, even when questions and type of respondents are held constant.

If the apparent instability of survey orientations results in part from lack of motivation and previous practice in making the relevant linkages between underlying preferences and articulated opinions, one would expect to find higher stability of values among the more educated and those most interested in societal problems. We do. In the American seven-year panel, my values index shows stability coefficients of .33 for those who say they are not interested in politics and .40 for those who are very interested. Stability is also greater among the more educated: among those with an elementary school education the coefficient is .19, while among those who attended college, it is .46. Van Deth (1983) reports similar findings from a Dutch panel study of these values.

The panel surveys provide yet another indication that the apparent instability of mass orientations is the result of measurement problems rather than nonattitudes. In two of these surveys, the 12-item materialist/postmaterialist values battery was included, as well as the four-item battery (which is one of its components). The broader-based index shows significantly greater stability over both short and long periods of time, probably because it measures the underlying orientation with greater accuracy. These findings

are consistent with previous results, which demonstrated that the 12-item index also manifests greater constraint with other attitudes than does the four-item index (Inglehart, 1977); both stability and constraint rise as one obtains greater accuracy of measurement.

Moreover it is not only under classroom or experimental conditions that the general public can be motivated to show high levels of constraint in their political attitudes. Stokes (1963) has suggested that under exceptional circumstances, such as a political crisis that evokes intense political polarization, strong ideological focus may be found among mass publics. One such case has been observed in France, in the spring of 1968. In the context of a series of strikes that totally paralyzed France, together with massive demonstrations and the occupation of factories and public buildings, a majority of the French public thought they might be on the brink of civil war. There was an enormous outpouring of political discussion. This situation of extreme politicization gave rise to remarkably high levels of constraint, with correlations as high as .70 being observed among the political attitudes of the French public (Inglehart & Hochstein, 1972).

This was a truly exceptional situation. Although there was a good deal of political activism in the United States at the same time, it was limited to relatively small groups. In France, on the other hand, practically no one was working in May and June 1968. Fully 20% of the sample reported having taken part in protest activities, and virtually everyone was talking about the crisis.

An exceptional situation produced exceptional levels of constraint, as Table 3 demonstrates. Among the American public, observed linkages between one's political party preference and one's stance on current political issues were at their usual modest levels. In France, they were incomparably higher. Among the American public, the mean correlation between voting intention and the five attitudes shown in Table 3 was .29. Among the French public, the mean correlation with five similar attitudes was .59. More recent surveys of the French public show constraint levels closer to the usual pattern, but the evidence suggests that under favorable conditions mass publics can manifest high levels of constraint.

Conclusion

Converse's basic insight still stands; there is a significant random component that must be recognized and dealt with in survey research. But the Black and White model, with its implications of widespread nonattitudes, is no longer tenable.

Table 3. Issue Constraint in France and the United States, 1968

	a
United States	
Favorable rating of President Johnson	.47
Favorable rating of conservatives	-.27
For racial integration of schools	.25
Favorable rating of liberals	.25
Favorable rating of labor unions	.23
France	
Favorable rating of President de Gaulle	.72
Favorable to Gaullist demonstrations	.67
Satisfied with government policy toward students	.54
Favorable to 1968 strikes	-.54
Favorable to labor union demonstrations	-.50

^aCorrelations between political party preference and issue positions.

For the orientations of Western publics show impressive durability, based on latent attitudes that prove to be much more stable than given survey indicators might seem to suggest. These latent attitudes can now be estimated relatively accurately through recent statistical techniques such as LISREL, and the results undermine the credibility of a Black and White model. Interestingly, in a recent analysis of the persistence of political party identification, Converse himself applies a Latent Attitude model (Converse & Markus, 1979).

Moreover, the stability of these latent attitudes does not exist only in the estimates of structural equation models. Its effects are visible when we examine certain kinds of aggregate survey results. Throughout Western society, younger birth cohorts are a great deal more likely to emphasize postmaterialist values than are older cohorts. Direct evidence demonstrates that these differences persisted throughout the period from 1970 to 1984, and cohort analysis indicates that they do not reflect life-cycle effects, but rather intergenerational differences. This fact, combined with other evidence (Inglehart, 1977, 1979), strongly suggests that these differences reflect the persisting impact of distinctive formative experiences that (in the case of the oldest cohort) took place as much as 50 years ago. Similarly, responses to the Rokeach terminal values battery show modest individual-level stability, together with an almost incredibly high aggregate stability, skewed in ways incompatible with assumptions that random answers are being given by anything more than a small fraction of the public.

The conclusion seems inescapable: mass publics have highly stable sociopolitical orientations that survey indicators measure imperfectly. We seem to be dealing with two levels of orientations in

which the responses articulated to given questions do not have a one-to-one relationship with relevant underlying affective orientations. If they have not sorted out these linkages before the survey, some respondents may give random answers to certain questions, especially if they are answering numerous questions in a limited time.

If the correlation between observed survey response and underlying attitude is .7 (roughly the level that applies to the data examined here), then the observed correlation between responses to two such items will be less than half as strong as the correlation between the underlying attitudes. This fact helps explain a pervasive finding that with survey data, relatively "strong" correlations between attitudes generally range from .3 to .45. With aggregate statistical data, the "strong" correlations range from .6 to .9, with the result that models based on economic indicators tend to explain about four times as much variance as those based on survey data. However, when one aggregates survey data to the level of the cohort, as was done above (cf., Dalton, 1977; Inglehart, 1979) or to the level of a given party's electorate (Inglehart, 1984), one explains about as large a percentage of the variance as one does with economic indicators; at the aggregate level, the random noise in survey data cancels itself out.

Thus, in some respects the aggregate results convey a more accurate view of the orientations of individuals than individual-level survey data do. Only by combining insights from the two perspectives can one obtain the full picture. For especially when one deals with topics on which the respondent has not articulated a position before the interview takes place, survey research systematically underestimates both stability and constraint in mass attitudes.

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The Political Bases of Citizen Contacting: A Cross-National Analysis

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This article examines a mode of political participation that frequently has been overlooked—individual efforts to obtain assistance from government officials. Using the seven nation data set of Verba, Nie, and Kim, we develop and empirically evaluate alternative models of citizen contacting. Our first model draws on variations in the distribution of social and economic resources to explain the likelihood of contacting. The second focuses on differences in political ties to locate those most likely to contact government officials. We find greater support for the political ties model. Persons active in political parties and election campaigns are the most likely to engage in citizen contacting. Without political ties, few poor or uneducated persons would ask officials for assistance. We conclude by noting the more general theoretical and normative implications of our study.

Political parties stand at the center of political participation. They form and reflect the preferences conveyed to government officials; they tie individuals into political blocs, maximizing the influence of each person and the group as a whole; they direct the activities of persons seeking to control governing positions and influence government policy. Political parties also are central to the mass mobilization types of political activities (voting, campaigning, demonstrating, and rallying). Finally, a link that has been little appreciated, they are crucial to citizen contacting, a mode of political behavior that differs sharply from other forms of political participation.

Citizen contacting involves individuals who approach government officials or other powerful persons in order to obtain help for themselves, their families, associates, or larger social groups. Private, face-to-face, and almost always designed

to assist a particular and limited set of persons, it is conceptually distinct from other forms of political participation. Citizen contacting does not involve the selection of office holders, does not include public demonstrations of political strength, and does not require the formation and maintenance of ties among claimants, whether through political parties, interest groups, or other political associations. It hardly ever seeks resources for groups larger than neighborhoods. The intimate bonds, sporadic encounters, and narrow demands of citizen contacting set it apart from the more frequently studied forms of political participation associated with mass mobilization, such as elections, rallies, and demonstrations.

Examples of citizen contacting abound. In relatively industrialized countries, a variety of persons may be expected to seek governmental assistance: unemployed workers looking for jobs, veterans with questions about military benefits, social security recipients in search of lost checks, builders seeking government contracts, and property owners angling for tax abatements are obvious instances. In developing nations, the list of contactors would also include peasants looking for access to markets, new city dwellers trying to adjust to their new living environment, and parents trying to keep their sons out of the army or looking for recommendations to help their children into universities.

Cross-national studies point to the ubiquity of citizen contacting as a mode of political participation. Barnes and Kaase (1979) provide evidence of contacting in eight industrial democracies: Austria, Finland, Great Britain, Italy, the Netherlands, Switzerland, the United States, and West Germany. They find that between one-

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fourth and one-third of the respondents in the European democracies and one-half of the American respondents have contacted government officials and other politically powerful persons. In their study, they show that about the same portion of these societies have contacted as have attended political meetings, and that more persons have voted than have engaged in other forms of participation. Verba, Nie, and Kim (1978) have also provided systematic information on contacting in three of these countries, Austria, the Netherlands, and the United States, as well as three others, India, Japan, and Yugoslavia. They found that between one-tenth and one-third of those surveyed engaged in citizen contacting, fewer than the proportion who voted, took part in election campaigns, and who were members of communal associations. Given the results of these and other studies (see, for example, Cornelius, 1978), it is clear that contacting is a form of political participation found everywhere and engaged in by significant portions of each society.

The significance of contacting rests on more than its ubiquity. No matter how ephemeral any one encounter between citizen and government official, taken together the interactions affect the pattern and content of policymaking. The more widespread contacting is within a country, the more time government officials spend responding to narrow demands. The more successful contacting is, the more government policies center around the production of private goods and public goods with very limited sets of beneficiaries. Studies of urban politics in the United States have emphasized especially the relationship between the delivery of government services and citizen contacting (Crenson, 1983; Eisinger, 1976; Jones, 1981; Jones, Greenberg, Kaufman, & Drew, 1977). Analyses of national politics have shown that responding to the casework demands—contacting's label as seen from Congress—takes up increasing portions of legislators' activities (Fiorina, 1977, 1982).

The structural implications of contacting increase further where the encounters are elements of ongoing relationships between citizens and public officials, as in the case of political clienteles and political machines (Schmidt, Scott, Lande, & Guasti, 1977; Zuckerman, 1979). Here the effort to obtain government resources is part of an established exchange relationship that frequently serves as the basis of party ties and other political activities. In these circumstances, contacting becomes a primary mode of affecting policy decisions, and success in contacting demands membership in a political clientele. To the extent that political machines and clienteles abound in a polity, government policy is likely to be particularistic. Evidence from local and national politics

in India, Italy, and Japan provide much support for these hypotheses (Di Palma, 1977; Nicholson, 1978; Zuckerman, 1979).

These patterns raise several analytical questions: What accounts for the cross-national and within-nation variations in contacting? To what extent do factors that explain voting, demonstrating, rioting, and other forms of political mobilization also account for contacting? Are the economically well-off and the educated especially likely to seek government resources? Does activity in a political party increase the likelihood of contacting? Does the type of party or party system affect citizen contacting?

In this article we explore the social and political correlates of contacting by examining two analytical models. The first draws on variations in the distribution of social and economic resources to explain the likelihood of contacting. It takes its theoretical strength from the ability of hypotheses using these variables to explain respectable—if not especially high—proportions of the variations in other forms of political participation (See, for example, Barnes & Kaase, 1979; Brody, 1979; Milbrath & Goel, 1977; Nie, Powell, & Prewitt, 1969; Verba, Nie, & Kim, 1978). The second approach views political connections and obligations as distinct from social and economic resources and draws on variations in ties to political parties to explain incidences of contacting. We test these arguments with data from Verba et al.'s (1978) study of political participation. The diverse set of cases enables us to use a different systems logic of comparison and thus strengthens the generalizability of our results. We find that persons active in political parties and election campaigns, that is, those with political ties to those who can help and the political obligations to help others, are most likely to engage in citizen contacting. We use these findings to draw more general theoretical and normative conclusions.

Alternative Models of Contacting

The most widely used approach to the explanation of contacting relates this form of political participation directly to an individual's social and economic resources. Although Verba et al. partially remove contacting from their analysis, the logic of their argument implies an explanation of this form of political participation as well. Their model contends that there is a direct, strong relationship between the level of social and economic resources and political participation, except where political institutions intervene. Under what conditions do political institutions affect that relationship? They do so where political participation is tied to intergroup conflict and where the costs of individual participation are low. Thus, Verba et

al. predict and find a strong relationship between membership in and psychological identity with political parties and political participation obviating the effect of socioeconomic level with regard to voting, a weaker one with campaign activities, and weakest of all for participation in communal associations. The conditions associated with contacting are the reverse of those associated with voting: high personal cost and low intergroup conflict. Hence, the Verba et al. model implies that citizen contacting is a direct function of socioeconomic level. The higher the level of social and economic resources, the more likely a person is to approach a politically powerful individual for assistance. A high level of social and economic resources is associated with high "issue neutral motivations" to participate: efficacy, interest, and obligation. Hence, political institutions will have no impact on the level of contacting, when controlling for level of socioeconomic resources (Verba, 1978). Their model makes no distinction between the influence of social and economic resources on contacting for oneself and one's family and for broader social groups. Hence, the strong and linear relationship between social and economic resource level and contacting applies to all forms of contacting.

Most studies of this form of political participation omit the political institutions and personal incentives that might encourage citizen contacting. Indeed, Jones's (1981) work on Chicago is the only analysis of contacting to use a characteristic of the political structure as an explanatory variable. Jones demonstrates that the probability that individuals in a neighborhood will engage in contacting is directly affected by the strength of the Democratic party in the area. Because Jones views the party structure of Chicago as atypical, he denies that the conclusions derived from this analysis may be extended to other cases (p. 699). An alternative explanatory model of contacting would argue that, far from being atypical, the pattern found in Chicago exemplifies a widespread association between political parties and other political institutions and contacting.

Furthermore, the approaches drawing on social and economic variables take no account of whether the person considering contacting believes it likely that he will succeed in affecting the political influential or whether the potential contactor has personal incentives or political obligations to help others. They omit the likelihood that some individuals will be singled out for help in contacting the government. They ignore too the widespread common sense wisdom that personal political connections and ties bring people to knock on the doors of the powerful, not need or high social and economic resources.

Our approach draws on the asymmetric dis-

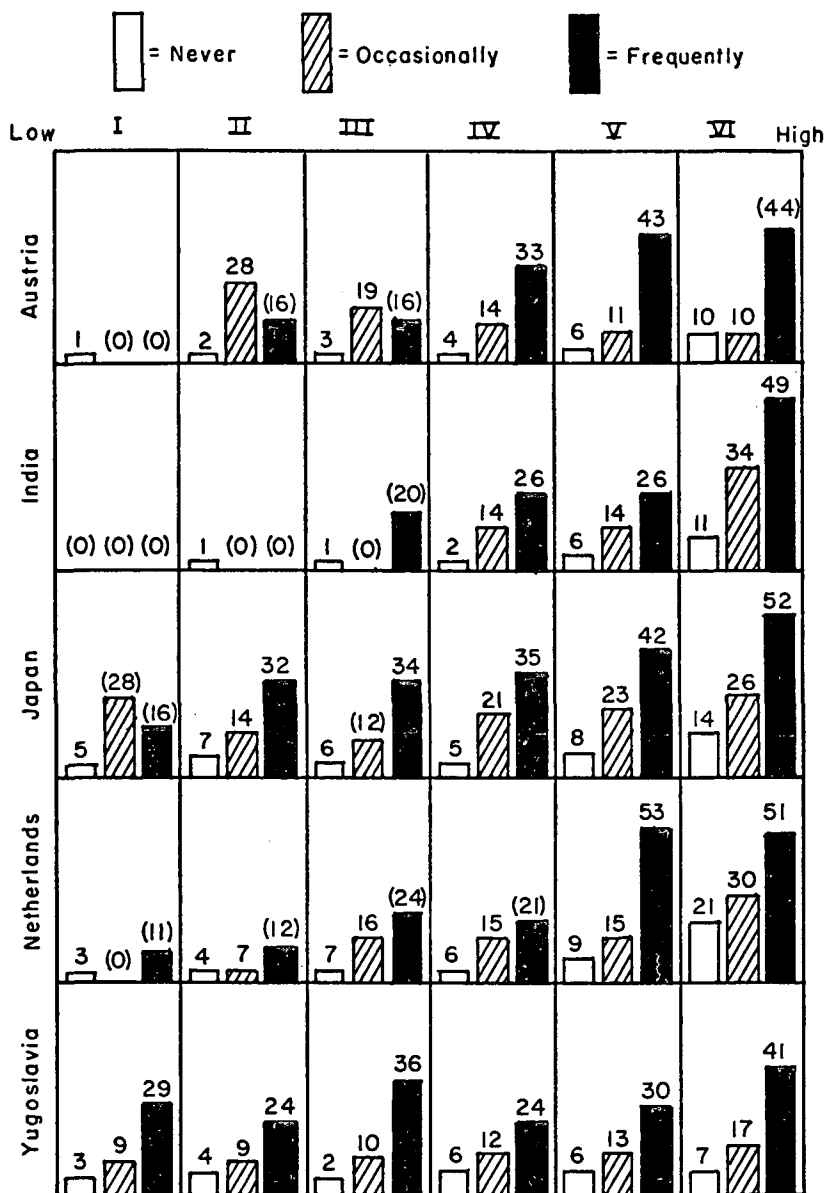
tribution of political contacts and obligations present in societies. Some individuals perceive it likely—and are so perceived by others—that their contacts with powerful persons will bear fruit. In peasant societies, landlords, lawyers, physicians, and school teachers fit this role. More generally, anyone with a direct personal bond to government officials qualifies. In addition, some individuals occupy roles and positions that obligate them to help others. Local campaign and party activists, whether ward heelers or local patrons in parties organized around political machines or others searching for votes, may be classified here. Two elements drive this approach to the analysis of contacting: personal ties to the government official which obligate the politically powerful person to the contactor, and political career or role responsibilities, which require the incumbents to contact power holders. This perspective views contacting as the result of a relationship, of an existing political bond, not a personal characteristic, such as education, income, efficacy, or need. It implies a set of alternative hypotheses that explain the likelihood of contacting: there is a direct relationship between political ties and contacting; the greater the number and strength of political ties and obligations that a person has, the more likely he is to approach a government official to help himself or others; and this relationship holds at all social and economic levels. Party activists are especially likely to engage in contacting because they have the political resources that induce positive responses from office holders. In addition, there is reason to argue that political ties have a greater impact on contacting to help people outside the immediate family. Political activists have the political connections that generally smooth the way to power holders and increase the probability of success. They also know that using those connections to help friends, neighbors, workmates, and others is likely to rebound to their own and their political associates' benefit and that not engaging in social contacting is likely to harm them. Hence, political ties will have a greater effect on the likelihood of contacting to help friends, neighbors, and other social groups than to assist the contactor himself or his family.

Data and Method

In order to test these models of citizen contacting in Nigeria, we did not analyze that country.) study conducted by Verba et al.¹ These surveys contain a wealth of information about contacting

¹For details about sample size and questionnaires in each country, see Verba and Nie (1972) and Verba, Nie, and Kim (1978).

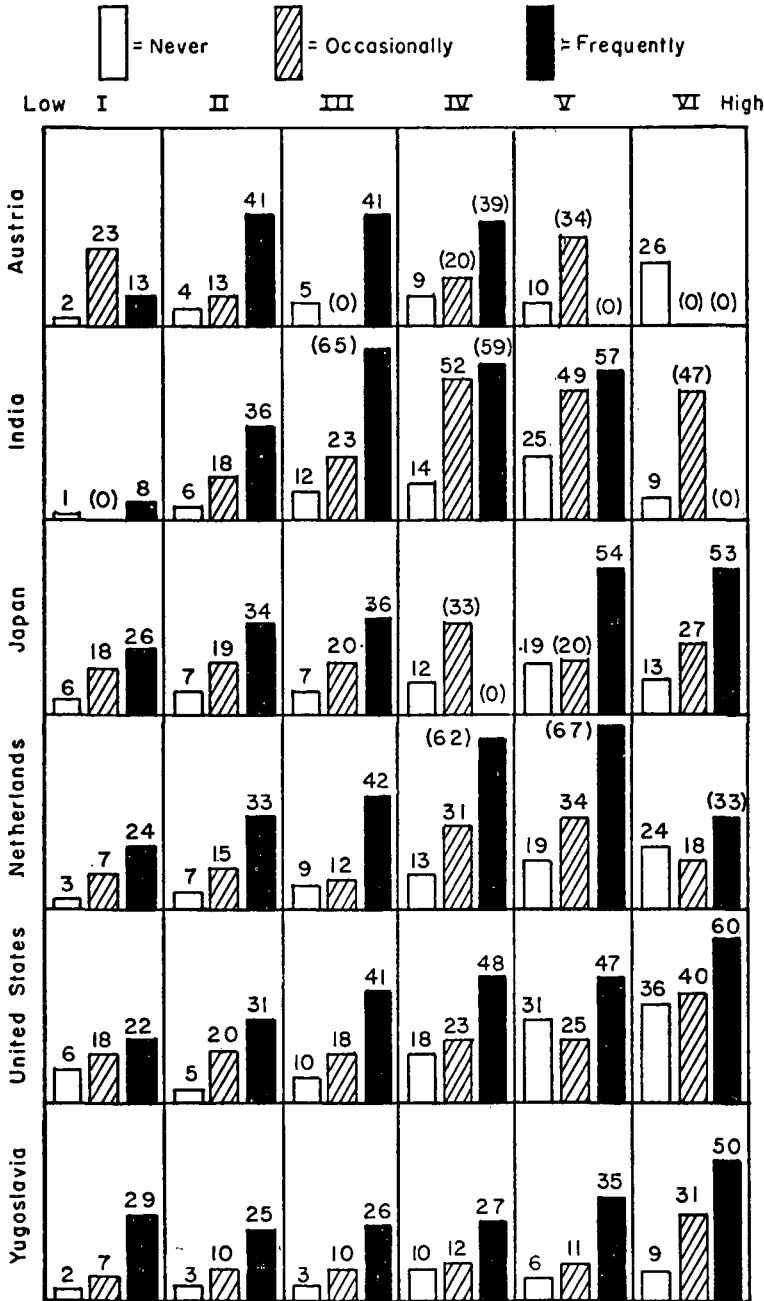
Figure 1. Proportion Who Do Social Contacting in Each Level of Socioeconomic Resources, Controlling for Campaign Work



Percentages in parentheses are based on ten or fewer respondents.

Note: Campaign activity maintains its statistical significance with contacting at the .05 level for the following socioeconomic levels: Austria (II-VI), India (III-VI), Japan (I-VI), Netherlands (III-VI), and Yugoslavia (I-VI).

Figure 2. Proportion Who Do Social Contacting in Each Level of Education, Controlling for Campaign Work



Note: Campaign activity maintains its statistical significance with contacting at the .05 level for the following socio-economic levels: Austria (I-III), India (II-IV), Japan (I-III, V, VI), Netherlands (I-IV), United States (I-V) and Yugoslavia (I-VI).

as well as other forms of participation. They permit us to distinguish among kinds of contacting and to relate them to activities of mass political mobilization. We investigated contacting in six of the countries included in the seven-nation study: Austria, India, Japan, Netherlands, United States, and Yugoslavia. (Owing to survey and data limitations and the paucity of citizen contacting in Nigeria, we did not analyze that country.) The political, economic, social, and cultural diversity of these six countries strengthens the theoretical power of the conclusions of our analysis.

Our study examines two dependent variables: particularized contacting and social contacting. The variables are derived from answers to survey questions about whether or not the respondent has contacted local or extralocal government officials or other powerful persons for help in addressing the needs of the respondent, his family, friends, neighborhood, workmates, community, or other social group, during the preceding two years. Particularized contacting combines local and extralocal efforts to help the contactor and his family. Social contacting refers to those who seek assistance for friends and others, whether locally or outside the local community. The labels particularized and social connote the nature of the beneficiaries, not the characteristics of the resources sought. The data show that both particularized and social contacting seek benefits for a distinct and narrow set of persons.

We have provided two sets of operationalizations of the key social and economic variables. The first is the measure of socioeconomic status that Verba et al. use in five of the six countries examined (only in the United States is this measure lacking). It ranks individuals by a combined measure of income and education that distinguishes six levels on a cross-culturally equivalent scale. The second measures the level of formal education completed, ranging from no schooling to university completion. This scale provides a measure for each society that may be applied to all six cases. In both operationalizations, the scales vary from the lowest to the highest.

The operationalization of political ties proved more difficult. The diverse set of political systems examined—a one-party Communist system, two multiparty democracies, two dominant party democracies, and a two-party democracy—introduced one source of problems. There are few indicators that both provide manageable tasks for survey analysis across the data sets and tap comparable phenomena in the different cases. The most intuitively plausible indicator, party membership, had to be excluded because the American and Japanese surveys contain no infor-

mation about it. Factors that locate persons with close personal ties to powerful individuals, such as those of family or friendship, were excluded because they amount to a very small portion of any society. Our measure examines the participation of individuals in the work of political parties. In the plural party systems, it examines activities in electoral campaigns by tapping the answers to the following questions: "Have you worked (frequently, occasionally, or never) in an election campaign in the last three years, such as distributing leaflets or playing an active role in campaign rallies?" In the case of Yugoslavia, we ascertained whether or not the respondent has been involved in the nominating procedures used to select government officials. The various measures tap the same underlying dimension of political connections and denote as well significant portions of each sample.

Our analysis relies primarily on a cross-tabulation technique which presents our findings in a clear and straightforward manner. It is especially appropriate because of the dichotomous nature of our contacting measures and because relatively few people engage in contacting. However, to strengthen our claims, we also utilized a multivariate approach. Those results, reported later in this article, generally matched our cross-tabulation findings.

The Analysis of Contacting

Our analysis begins by exploring contacting across the six levels of socioeconomic status. We first cross-tabulated socioeconomic resource level and social contacting. Although the magnitude of the relationship varies cross-nationally, the results show a direct relationship between the distribution of social and economic resources and the incidence of social contacting. These patterns are especially striking in the Netherlands, where the level of social contacting varies from 3% for those in the lowest socioeconomic categories to 25% for those in the highest, and in India, where the level varies from 0 to 16%. The relationship also holds in the other countries: Austria (from 1 to 11%); Japan (from 7 to 20%), and Yugoslavia (from 7 to 18%). In the United States, the comparison across income groups shows a similar relationship: 5% of those earning less than \$1000 per year have done social contacting, whereas 40% of those earning \$25,000 or more per year have engaged in this form of political participation. In all cases, the higher the level of socioeconomic resources, the greater the amount of social contacting.

In Table 2, we present the effect on social contacting of an alternative measure of socioeconomic status, education. Again, the contrasts be-

Table 1. Incidence of Social Contacting at Six Socioeconomic Levels (%)*

	Low 1	2	3	4	5	High 6	Mean	N
Austria	1	4	4	7	9	11	6	1744
India	0	1	1	4	7	16	5	2554
Japan	7	11	10	12	15	20	13	2620
Netherlands	3	4	9	9	13	25	10	1526
Yugoslavia	7	8	10	11	14	18	12	2312

*Cell differences are statistically significant at the .05 level.

tween those at low and high levels are striking. The strength of the relationship varies from a six-fold increase (Netherlands and United States), to an eight-fold (Austria and Yugoslavia), and to a thirteen-fold (India) growth across educational levels. The highly educated are much more likely to contact government officials than are those with low levels of education. Furthermore, it is not just high education that adds to the likelihood of social contacting. Increases in social contacting occur with each additional level, high school, college, and postgraduate education.

There are various features of social contacting that might account for these relationships. By its very nature, this mode of participation involves awareness of others and their problems. Contacting government officials on behalf of friends, neighbors, and workmates presupposes interest in or familiarity with the plight of those in one's larger community. High levels of wealth, income, and education presumably increase both interest and familiarity, which in turn are likely to facilitate social contacting. In addition, socioeconomic status and education probably equip contactors with the financial and intellectual resources needed to request assistance from government officials.

The patterns between these explanatory variables and particularized contacting are much weaker and less clearcut. A comparison of Tables 3 and 4 shows that socioeconomic status and education have weak links to contacting for one's self and family. In Table 3, increasing levels of socioeconomic resources are associated with strongly increased particularized contacting only in India (from 7 to 28%) and do so to a much lesser extent in Austria (from 17 to 22%). In the other countries, the results show either no difference across socioeconomic levels or slight drops among those respondents at the highest categories. Table 4 documents similar patterns for educational groupings. India shows an increase from 9 to 32% across the levels of education. In none of the other countries is there a direct relationship. Particularized contacting is much less influenced by increasing amounts of income, wealth, and education than is social contacting.

What is especially striking about these results is the absence of any systematic patterns concerning particularized contacting. In the Netherlands, the relationship between the explanatory variables and contacting for self and family is best described by a flat line; in India, it rises directly with increasing levels of income, wealth, and educa-

Table 2. Incidence of Social Contacting at Six Levels of Education (%)*

	Low 1	2	3	4	5	High 6	Mean
Austria	3	7	7	13	12	25	6
India	1	8 ^a	17	23	32	13	5
Japan	9 ^a	12	13	14	26	20	13
Netherlands	4	9	11	21	25	23 ^a	10
United States	11	15	21	30	37	51	20 ^b
Yugoslavia	4	8	9	14	14	30	11

*Cell differences are statistically significant at the .05 level.

^aThis cell merges two levels.

^bThe sample size is 2524.

Table 3. Incidence of particularized Contacting at Six Socioeconomic Levels (%)

	Low 1	2	3	4	5	High 6	Mean
Austria	17	22	22	21	18	22	20
India*	7	10	10	17	18	28	15
Japan	7	8	9	10	8	10	8
Netherlands	36	33	33	36	32	34	34
Yugoslavia*	18	24	22	25	22	14	21

*Cell differences are statistically significant at the .05 level.

tion. In the other countries, the two measures have different impacts on the dependent variable: sometimes a parabolic effect appears, sometimes a direct relationship, and sometimes no relationship at all.

This evidence does not support two widely cited hypotheses. Particularized contacting is not a function of the interaction between the need for government assistance and the possession of information on how to obtain that help (Brody, 1979; Jones et al., 1977). The expected parabolic curve, locating those most likely to engage in this mode of contacting at the middle levels of income, wealth, and education, appears only in Yugoslavia. In addition, there is little support for the claim that particularized contacting relates directly to level of socioeconomic resources or need (Sharp, 1982; Thomas, 1982).

These results may reflect the parochial nature of particularized contacting. Requests for government assistance on behalf of one's self or family are especially likely to reflect idiosyncratic needs, and this form of contacting may reflect the essentially random nature of these incentives. It may also respond to the relative ease of contacting government officials and powerful persons. The well-developed Dutch welfare state makes seeking government aid equally easy for everyone; those at the lowest levels of income, wealth, and formal

education do particularized contacting more frequently than those in all the categories of the other political systems. By itself, need has no influence on the likelihood of seeking government aid for one's self and one's family. No group has a more generalized need for assistance than the poor and uneducated in India, yet no group is less likely to engage in particularized contacting. Both points reinforce the claims that this mode of contacting is directly affected by the idiosyncratic distribution of personal needs and the extent to which political institutions foster particularized contacting.

In Tables 5 and 6, we cross-tabulate political campaign work and the two contacting variables. Table 5 reports a strong relationship between the frequency of campaign work and social contacting. In all political systems, those who frequently engage in this form of political participation are much more likely to contact government officials for help than those who are not politically active. For example, only 5% of the Austrians who have never campaigned contacted officials on behalf of others, whereas 32% of the frequent campaigners did. Similarly, 3% of the Indians who are politically inactive contacted officials, whereas 34% of the actives did. This pattern is repeated in all six countries, which leads to the striking conclusion that one-third of those who frequently

Table 4. Incidence of Particularized Contacting at Six Levels of Education (%)

	Low 1	2	3	4	5	High 6	Mean
Austria	20	18	23	24	25	12	20
India*	9	23 ^a	29	37	35	32	15
Japan	5	8	9	10	9	11	8
Netherlands	33	33	36	36	35	37	34
United States*	8 ^b	9	12	13	14	10	10
Yugoslavia*	16	21	25	22	16	18	21

*Cell differences are statistically significant at the .05 level.

^aThis cell merges three levels of education.

^bThis cell merges two levels of education.

Table 5. Incidence of Social Contacting at Three Levels of Campaign Work (%)*

	Never	Occasionally	Frequently	Mean
Austria	5	14	32	6
India	3	22	34	5
Japan	7	20	37	13
Netherlands	8	16	37	11
United States	9	20	39	20
Yugoslavia	5	12	31	11

*Cell differences are statistically significant at the .05 level.

perform campaign work also engage in social contacting. Political activities strongly increase the propensity for social contacting in all political systems.

This relationship holds for particularized contacting as well, although the magnitude is not as strong. Table 6 shows a clear linear relationship between political contribution and particularized contacting. In India, only 13% of the inactives contact government officials for themselves and their families, compared to 47% of those active in election campaigns. In the United States and Japan, the likelihood of particularized contacting doubles (from 8 to 15% and from 6 to 17%) as one moves along the measure of campaign activity. Because this form of contacting generally responds to idiosyncratic circumstances and is likely, therefore, to have a random quality, the link between political networking and particularized contacting is especially striking.

Political ties facilitate contacting in different ways. Political activities help the formation of connections that open political doors in all political systems. They also increase a person's sense that contacting will succeed in remedying social or family problems. Finally, they are likely to increase the incentives to contact government officials by lowering the costs of the activities and raising the direct personal benefits. Political networking is important both for social and particularized contacting.

The evidence we have presented to this point describes two-way cross-tabulations. We have found that variations in socioeconomic resources are related to social but not particularized contacting and that political networking is related to both types of contacting. Important questions remain: Are political connections equally important for all people? More precisely, are they as important for those with high levels as for those with low levels of income, wealth, and education? How will the relationship between socioeconomic resources and contacting be affected when one controls for political work? Will the original patterns remain?

In order to address these questions, we cross-tabulated the measures of socioeconomic resources, education, political work, and the two kinds of contacting. Figures 1 and 2 show three-way cross-tabulations for social contacting, with several interesting results: First, political work consistently exercises a powerful impact on this form of contacting. In all political systems there is a strong relationship between the frequency of engaging in campaign activities and the likelihood of social contacting. Second, these effects are present in almost every category. Third, the boost provided by political activities is especially critical for individuals at low levels of socioeconomic resources. Without political activities, their contacting would be minimal (less than 5%). Fourth, there are interactive effects between political work

Table 6. Incidence of Particularized Contacting at Three Levels of Campaign Work (%)*

	Never	Occasionally	Frequently	Mean
Austria	20	28	28	20
India	13	41	47	15
Japan	6	13	17	8
Netherlands	33	35	49	34
United States	8	11	15	10
Yugoslavia	16	27	28	21

*Cell differences are statistically significant at the .05 level.

and level of socioeconomic resources and education. Persons most likely to engage in contacting are those who are politically active *and* financially well-off or well-educated. Approximately half these people engage in social contacting. These patterns raise important normative issues, which we discuss in the conclusion.

The powerful effect of campaign activities on social contacting at various levels of socioeconomic resources and education is demonstrated even more clearly in the analysis of social contacting at the local level, the most frequent locus of this mode of political participation.² Several patterns are consistently present: Persons who have the lowest levels of education and who have taken part in campaign work are more likely to contact than are those at the highest levels of education but with no campaign experience. Those at the lowest socioeconomic levels who have campaigned contact more frequently than those at the highest levels who have not done election work. In those categories of socioeconomic resources and education with the highest rates of contacting, adding campaign work strongly increases the frequency of contacting.

Some examples from the data illustrate these generalizations. In the United States, almost everyone who engages in social contacting at the local level is a campaign worker. In the two income categories most likely to engage in social contacting, 30% do so. Every contactor in these categories has worked in an election campaign. In the income category containing the next highest level of this form of political participation, 92% have worked in elections. In the United States, 82% of the persons who contacted local officials and powerful persons to help their friends, neighbors, workmates, or other social groups have taken part in election campaigns. In this political system, electoral connections appear to be a necessary condition for social contacting at the local level.

The effect of education on the likelihood of social contacting at the local level is most striking in India. Here, 65% (1642 respondents) of the sample were illiterate. Of these, only one person has engaged in local contacting for a social group, and he has also worked in an election campaign. Among Indians with some formal education, however, political work has a strong influence on the likelihood of social contacting. In the education categories containing the highest proportion of social contactors, adding campaign work doubles the frequency of contacting to 50%.

Among those who actually have engaged in social contacting, nearly 90% were campaign workers.

The analysis of social contacting at the local level demonstrates the powerful impact of political work across all categories of socioeconomic and economic resources. In this most frequent form of social contacting, political contacts open doors and provide the incentives for this kind of political participation.

Figures 3 and 4 present three-way cross-tabulations for particularized contacting. Again, the effect of political work here is not as strong as it is on social contacting. Political work has an effect in some political systems (India, Japan, and the Netherlands) and on many socioeconomic groupings (usually those at the lowest levels). There is some limited evidence of a parabolic effect in India, where the middle categories have the highest rates, even when controlling for political work. There is no evidence of a parabolic effect, however, in any of the other countries. Political work has the greatest impact on particularized contacting of all the explanatory variables that we have examined.

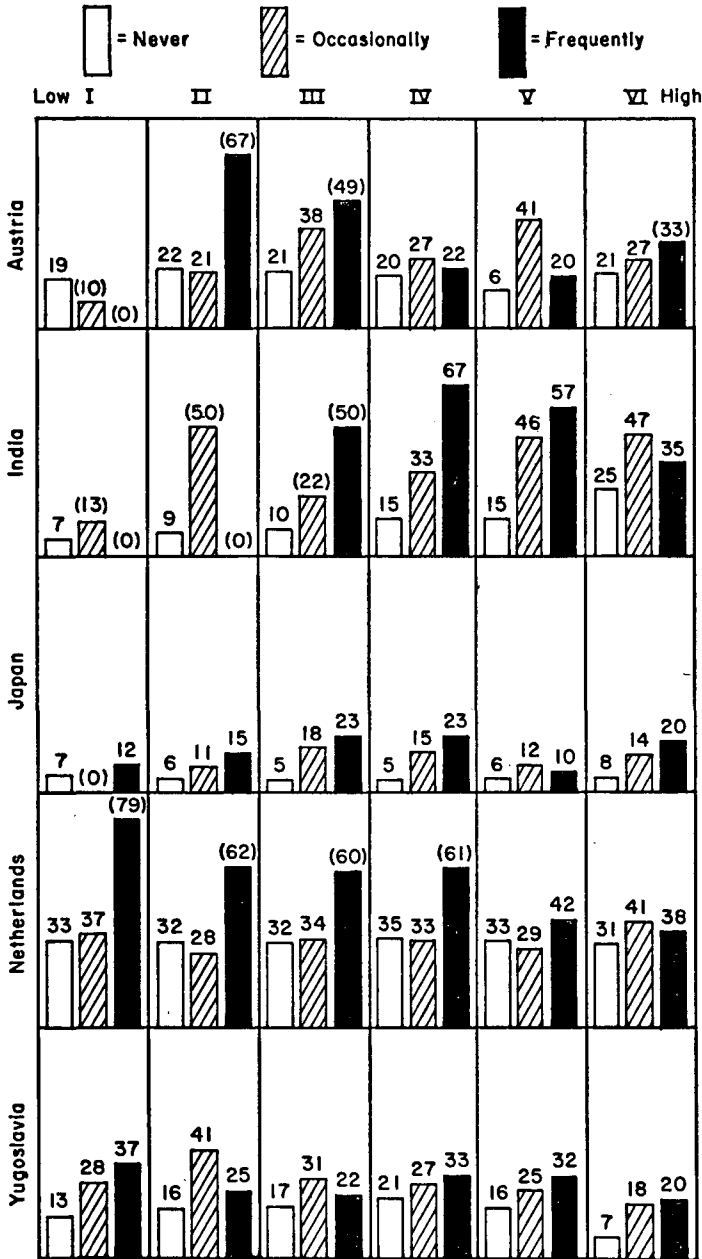
In addition, the power of the political variables is even stronger in the analysis of local contacting. Here too, those at the lowest socioeconomic levels who have taken part in election campaigns are more likely to contact local government officials for themselves and their families than are those at the highest levels, but who have no campaign experience. Similarly, those with the least formal education who have campaign experience are more likely to engage in contacting than those at the highest levels of education who have no campaign experience. The frequency of contacting among persons in those categories of wealth, income, and education most likely to contact is greater where campaign activity is greater.

Finally, we conducted a regression analysis of contacting patterns to determine whether or not the earlier patterns held true.³ We used particularized and social contacting as dependent variables and evaluated the relative influences of socioeconomic resource level, education, and campaign activity. Three conclusions stand out. First, like others who have studied citizen contacting (Barnes & Kaase, 1979; Nie et al., 1978), we found that our independent variables explained a relatively small percentage of the variance in contacting activity. Although there was variation from country to country, the general range was from 3 to 15% of the variance. Second, our model generally worked better as explanations of social than particularized contacting. This finding is

²The Yugoslav data do not distinguish between local and extralocal contacting.

³We also experimented with a log-linear approach and found similar results.

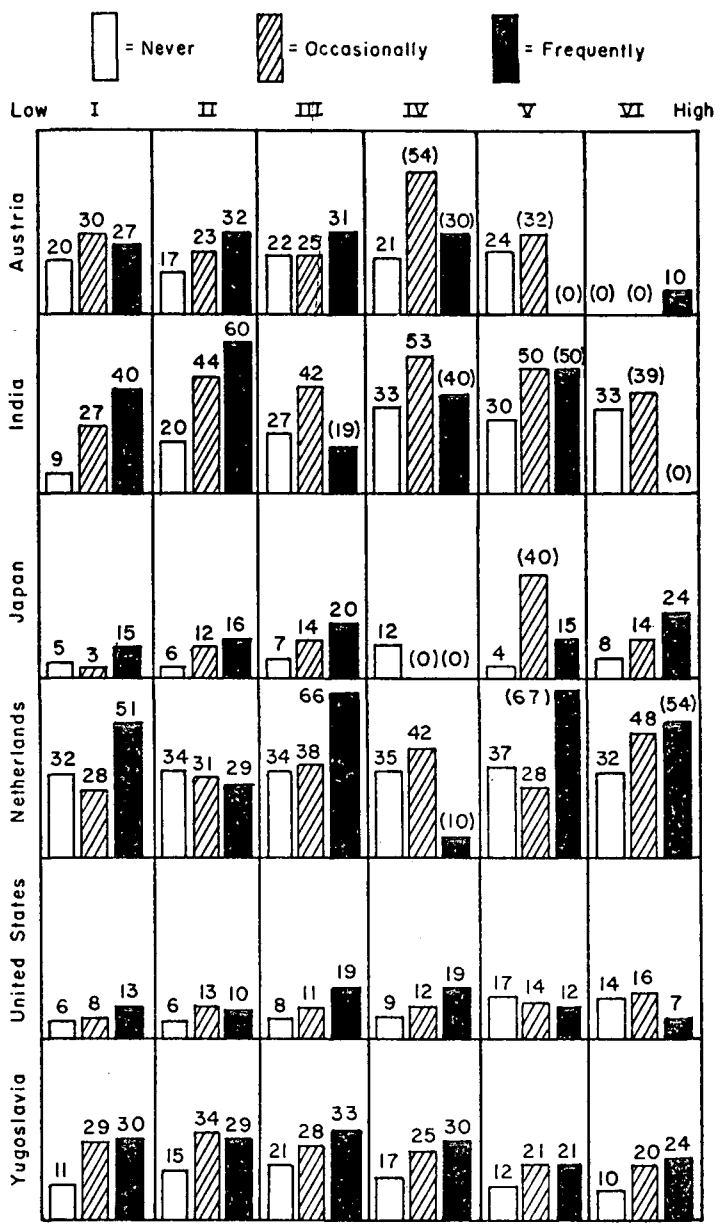
Figure 3. Proportion Who Do Particularized Contacting in Each Level of Socioeconomic Resources, Controlling for Campaign Work



Percentages in parentheses are based on ten or fewer respondents.

Note: Campaign activity maintains its statistical significance with contacting at the .05 level for the following socioeconomic levels: Austria (II-V), India (II-VI), Japan (II-III, IV, VI), Netherlands (I) and Yugoslavia (I-VI).

Figure 4. Proportion Who Do Particularized Contacting in Each Level of Education, Controlling for Campaign Work



Note: Campaign activity maintains its statistical significance at the .05 level for the following socioeconomic levels: India (I, II), Japan (II, III, V), Netherlands (III), United States (II, III), and Yugoslavia (I-III).

consistent with our cross-tabulation results and provides additional evidence of the idiosyncratic nature of particularized contacting. Third, the regression results confirm the greater explanatory power of political over socioeconomic variables. Participation in political activities such as campaign work accounted for almost three-quarters of the variance we were able to explain. The only exception to this pattern was India, where the powerful limiting effect of illiteracy made education account for the largest share of the variance.

In sum, our analysis indicates that the political networking model helps to explain variations in the level of contacting. Political activities, and the advantages they bring (connections, interest, and a sense of efficacy), facilitate citizen contacting of government officials. People who are politically active are much more likely to contact officials than those who are not active. Although there is some variation from country to country, these patterns appear consistently in a diverse set of political systems and under a variety of methodological approaches.

Political Networking: A Party Link

Our measure of political ties focuses on campaign activities without directly exploring the impact of political parties, the institution most involved in election campaigns. The absence of a measure of party membership in all six countries necessitated this less-than-completely satisfactory operationalization. Does including a direct measure of party ties increase the effect of political networks on social and particularized contacting? The data for India, Austria, Yugoslavia, and the Netherlands provide information on party membership that shows that campaign activists who are party members are still more likely to engage in both forms of contacting than those campaigners who are not tied to a political party. The relationship is especially strong, once again, with regard to social contacting.

Table 7 reports the variable impact of party and campaign ties on social contacting. The pattern is the same in all the cases. Approximately half the persons who are party members and frequent campaigners do social contacting. Each party and campaign factor has an added impact on this mode of contacting. In the Netherlands, party members who are frequent campaigners are five times more likely than other citizens to engage in social contacting; in Yugoslavia, they are nearly four times more likely; in Austria, they are seven times more likely, and in India, they are nearly ten times more likely.

In every country we studied, nearly half of all the people who do social contacting are party members, campaigners, or both. In the Netherlands, persons with these political ties are exactly 50% of the social contactors (26% of the sample); in Yugoslavia, they are 83% (49% of the total); in Austria, they are 51% (30% of the sample), and in India, they are 45% of those who do social contacting (and 5% of the total). The patterns are simple, clear, and consistent; there is a strong, direct relationship between political ties and social contacting.

The effect of party ties and political connections on particularized contacting is present, but not as strong. Table 8 indicates that in the Netherlands and India, half the party members who are frequent campaigners engage in particularized contacting. The proportion is considerably lower in Yugoslavia and Austria. In India and the Netherlands, these political ties raise the probability of particularized contacting to well above the national mean. The proportion that party members and campaigners represent among those who do this form of contacting varies across the political systems: In the Netherlands, they are 29% (27% of the sample); in Austria, they are 37% (30% of the total); in Yugoslavia, 61% (44% of the total), and in India, 51% (5% of the sample). As we noted in the analysis of campaign work, only in India do political ties have equally powerful effects on both forms of contacting.

Table 7. Incidence of Social Contacting at Levels of Party Membership and Capaign Work (%)

	Austria	India	Netherlands	Yugoslavia
Party members and frequent campaigners ^a	42	47	53	44
Frequent campaigners not party members	32	35	37	24
All party members	11	22	27	28
Party members no campaign work	6	14	20	15
Mean	6	5	10	12

^aIndividuals who have taken part in three or more election campaigns.

Table 8. Incidence of Particularized Contacting at Levels of Party Membership and Campaign Work (%)

	Austria	India	Netherlands	Yugoslavia
Party members and frequent campaigners ^a	28	56	50	25
Frequent campaigners not party members	28	47	49	29
All party members	28	44	38	24
Party members no campaign work	20	38	36	18
Mean	20	15	34	21

^aIndividuals who have taken part in three or more election campaigns.

Conclusions

Ties to political parties—as members and campaign activists—strongly affect the likelihood of contacting. This variable maintains its effect at all levels of social and economic resources; it makes the poor and uneducated more likely to contact than those who do not campaign; it further increases the likelihood of the wealthy and well educated to engage in contacting. Without political ties, the distribution of social contacting would look much like the distribution of socioeconomic resources, and the distribution of particularized contacting would have no form at all, reflecting the random nature of personal needs. Efforts to explain the likelihood of contacting need to include the distribution of political connections in the analysis.

Approximately half of those who take part in election campaigns and who are members of political parties knock on the doors of the politically powerful for governmental assistance. In turn, more than half of those who do the knocking are party and campaign activists. These connections go a long way in explaining who is likely to engage in contacting. These findings appear consistently in a politically, culturally, socially and economically diverse set of countries: Austria, India, Japan, the Netherlands, the United States, and Yugoslavia. They are, therefore, highly likely to appear elsewhere as well.

These findings have important implications. Political connections, especially to parties, emerge as the major structural basis for overcoming the limitations on political action imposed by poverty and ignorance (Schattschneider, 1960). Efforts to increase citizen demands on governments grow and are most effective when linked to political parties (Jones, 1981). If the place of parties has declined in the United States since these data were collected, then we would expect a concomitant fall in the level of social contacting (Eldersveld, 1982).

At the same time, party ties are by their very nature exclusive. They help only those who have the right political connections. The power of political clienteles in Indian and Japanese politics and policymaking is underlined by our finding that political connections have an especially powerful effect on all forms of contacting in these countries. The Netherlands provides an important counterexample with regard to particularized contacting. Here, the very high relative level of particularized contacting reflects the availability of government agencies that facilitate it. In the Netherlands, government structure weakens the exclusive quality of political connections as a determinant of particularized contacting. In all instances, political structures and connections directly and strongly influence the likelihood of contacting.

The variables that affect the relationship between political ties and contacting reflect neither the type of party system present nor the level of interparty competition. Similarly, the kind of party, as ascertained by organizational structure or ideology, has very little impact on levels of contacting within or among the political systems. Party ties and campaign activity have a direct and powerful effect on contacting, especially social contacting, regardless of the kind of party or party system.

The tendency for political ties to have a greater impact on social than particularized contacting raises a final general point. The differential effect reflects the additional benefits and costs faced by political activists, who have neighbors, friends, and workmates in need of governmental assistance. To help—to engage in social contacting—fulfills the expectations of these colleagues and cements the political bonds that bring votes to the activist's political allies. Not to help risks the loss of the political support of one's friends, neighbors, and workmates, and jeopardizes the activist's political base. The party and campaign activist is the focal point for the contacting efforts

of the people around him. Because he has the political connections and the political obligations, they turn to him for political assistance. This connection becomes the structural basis of the political ties that account for much of the social contacting that exists throughout the world.

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A Theoretical Analysis of the "Green Lobby"

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The purpose of this article is to develop a theoretical framework for analyzing why individuals support private environmental "public interest" groups. The model attempts to integrate past contributions which have argued that these decisions could be explained by one of several factors, including: bounded rationality and imperfect information (Moe, 1980); the features of the public goods provided (or influenced by) these groups (Mitchell, 1979); the existence of a disequilibrium in households' demands for public goods (Weisbrod, 1977); or as a response to contract failures (Hansmann, 1980). The article uses a theoretical framework originally developed to explain individuals' decisions to join private clubs and specifies the conditions for the efficient provision of access to different types of private, nonprofit groups. By describing the optimal access conditions as if individuals could be coordinated to assure this efficient outcome, the model provides insights into the benefits and costs associated with membership in the environmental groups in practice.

This article develops a theoretical framework for describing why individuals support private environmental public interest groups. Today these groups are increasingly regarded as an effective lobbying force in promoting environmental causes. Membership in environmental groups currently exceeds five million. Their collective annual budget was approximately \$85 million in 1982.¹ Moreover, a recent article in a popular business magazine on the so-called green lobby ranks its clout with that displayed by the right-to-life movement and the National Rifle Association.² Despite this increasing presence in environmental politics,

analysis of the reasons why individuals support these organizations remains quite limited. Early political science research adopted a pluralist explanation of interest groups, arguing that individuals choose to join such groups because they supported the group's goals. Olson's (1965) analysis of collective behavior provided a significant challenge to this approach. Rather than acting to realize common interests, Olson's framework implies that for individuals to join such groups, they must receive some type of selective incentives (i.e., gains that are private or subject to some form of exclusion).³

Three recent developments have contributed to the process of amending these views: 1) Moe's (1980) recent conceptual analysis of group behavior has argued that bounded rationality and imperfect information have important implications for description of group membership decisions and group activities in general; 2) Mitchell's (1979) conceptual and empirical analysis of the

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¹These statistics are taken from Symonds (1982).

²More specifically, Symonds (1982, p. 137) noted that: "Since President Reagan and the first Republican-controlled Senate in a generation took power in Washington, many of the liberal ideas and programs that flourished in the Sixties and Seventies have faltered. One that hasn't is the environmental movement, which has not only hung on to its public support but become so much stronger and more sophisticated that it deserves being ranked with the National Rifle Association and the right-to-life movement as a superlobby."

³Mancur Olson's (1965) analysis of the economic incentives for collective action suggests that a rational, self-interested, economic agent must be given specific, private (or in his terms, "selective") inducements to join a group. The provision of a public good (directly or indirectly) as a result of the group's activities, without the ability to exclude nonmembers from its enjoyment, will not be sufficient motivation to sustain a large pressure group.

These groups have also been described as pressure groups in the literature in political science and sociology dealing with their activities. See Castles (1967) and Truman (1958) as examples of the early literature and Moe (1980) for a more recent treatment of the political influences to group behavior.

motivations for individual membership in environmental groups provides the rudiments of an alternative explanation of the growth of these organizations; and 3) advances in modeling of private, nonprofit organizations have provided insights that can be used to enhance our understanding of an individual's motives for supporting environmental groups (notable examples include the work of Weisbrod (1977), and Hansmann (1980, 1981)).

The present model attempts to integrate these earlier contributions with an amended form of the theoretical model used to describe individuals' membership decisions for private clubs designed to provide local public (or collective) goods.⁴ More specifically, it describes the conditions for an efficient provision of access to different types of private, nonprofit groups. While the framework describes an optimal plan for access conditions as if the decisions of individuals could be coordinated to assure the efficient outcome, it does nonetheless provide insights into the relevant tradeoffs between the benefits and costs associated with membership in these groups in the real world, where the conditions governing access to each type of organization are not the result of an optimal plan.

This article is developed in four sections. The first of these reviews the relevant aspects of past analyses of the motivations for the formation of nonprofit groups and for their behavioral decisions. This includes a summary of both Moe's analysis of group behavior and Mitchell's specific survey findings on the reasons why individuals join environmental groups. In addition I consider the role of Weisbrod's (1977) and Hansmann's (1980) analyses of nonprofit organizations, together with other related evaluations of this sector (notably Downing and Brady (1981), Holtmann (1983), and James (1983)). The second section outlines a generalized model of club membership that recognizes the range of choices available to the individual in satisfying his or her demands for environmental amenities. For the purposes of my analysis these are represented by a society-wide environmental collective good, a privately available substitute good, and the services provided by

two distinct types of nonprofit organizations. The first offers a club-specific service which is a collective good to members and capable of being denied to nonmembers. The second is a lobby; it offers members the prospects for influencing the level of provision of the society-wide collective good that is perceived to be available to its members.

As a rule, public interest groups rely on donations to support their activities. For example, Downing and Brady (1981) report that 26% of the total revenue of the Sierra Club in 1979 was derived from contributions.⁵ Membership fees in the same year accounted for 34% of revenue. Hansmann (1981) has suggested that for many nonprofit organizations, the combination of fees for membership (or services) and donations can be interpreted as offering the prospects for voluntary price discrimination. Therefore, in the fourth section I discuss the implications of donations as a means of support for environmental lobbies. The last section summarizes the results of this analysis and discusses their implications for understanding the political economy of environmental groups.

The Rationale for Private Public Interest Groups

Nearly all of the current paradigms offered as descriptions of group behavior find their origins in Mancur Olson's (1965) work. Olson's analysis suggested that such groups provide members with two types of goods (or services): a private good and a collective good. However, in his framework it was suggested that this public good had to be largely a secondary consideration to the membership decision, when the ability to prevent other individuals from having access to it was not possible. Indeed, Olson's argument indicated that individuals will tend to act as free riders, seeking to enjoy the benefits that the organization provided without sharing in its costs. This tendency was enhanced within large organizations; that is, there might be large costs for large groups to provide public goods (because of the number of individuals involved), but we can generally expect to find that each individual's share of the costs of provision would be small. As a result, the individual can easily interpret this small share as un-

⁴The term "local" implies that there are boundaries to the public character of the good or service that permit individuals to be excluded from access. In the case of a pure public good, each individual's consumption of the good would not impinge on the ability of others to consume it, and there would be no ability to exclude these individuals from access.

As a rule, this term is used for municipal services, which, it is argued, have certain public good attributes and also provide some basis for exclusion.

⁵This estimate is substantially greater than the crude estimates derived as part of the discussion in the fourth section. Comparisons of Mitchell's per-member payments with posted membership fees would imply donations of only 1% of total payments. The statistical information on these groups is quite limited. The Downing-Brady data are likely to be more accurate indicators of donations. Unfortunately comparable data were not available for the other groups.

important to the continued provision of the public good. Olson referred to this difficulty as the inconsequentiality problem. The existence of these types of judgments on the part of individuals serves to provide reinforcement to the free rider tendency through a tyranny of small decisions.⁶ Accordingly, in Olson's framework group, actions leading to the provision of some public good as an indirect by-product of their activities could be expected only where there was an ability to coerce individuals to join the group, or sufficient individual private services were provided to each individual to offset the costs of membership.

Although the general conclusions of Olson's analysis are now generally accepted, there have been important initiatives to amend the framework to admit a wider range of goals than those implied by a purely economic perspective. In many respects these efforts seek a middle ground between the early pluralist explanations of group formation and the rational economic calculus embodied in the Olson model.

This research has considered several issues. The issue most important to these objectives involves the role of group size in its behavioral decisions and the implications of the idealized assumptions of the Olson model for its conclusions. Considering the first of these, Hardin (1977) has suggested that altruism and a concern for group goals are possible, but only within small, intimate groups. Indeed, consistent with Olson's model, he assigns a limited role to large groups in responding to such goals, noting that:

In large groups social policy institutions necessarily must be guided by what I have called the Cardinal Rule of Policy: *Never ask a person to act against his own self-interest.* It is within the limitations of this rule that we must seek to create our future. . . . In the intimacy of small groups altruism may be substantial and important; in large groups enlightened egoism is the most powerful motive. (Hardin, 1977, p. 27)

Olson admitted this possibility by recognizing

"The concept of lobbying activity is being used rather loosely in this discussion. Legal statutes governing the behavior of nonprofit organizations limit the roles they can play in influencing political decisions. Thus, the reference to lobbying activities is not in the terms required for tax-exempt status; rather it is intended to refer to those sanctioned actions that direct public attention to environmental issues, commentaries provided by environmental groups during prescribed public review and information sessions, and other recognized open-access forums. Although such meetings are, in principle, equally accessible to individuals, the available information and costs generally preclude individual action and favor group responses.

that small groups could, in fact, provide empirically important levels of a collective good. Although some environmental groups have been small, their recent political successes have been largely the result of the coalition of these groups in responding to national environmental issues—actions that move in the opposite direction to that implied by these group size arguments.

The second line of research extending Olson's work focuses on his assumptions of perfect information and rational economic behavior. Moe's conceptual and empirical analyses of a variety of groups provided strong support for the importance of economic considerations as a dominant but not the exclusive explanation of behavior. He concluded his empirical appraisal noting that:

The overall impression conveyed by the interest group studies is simply that the average member ascribes more importance to nonpolitical than to political inducements and that he primarily seeks economic gain rather than purposive or solidarity incentives. (Moe, 1980, p. 200)

However, other motives, including altruism, status, and prestige do play some role as secondary considerations in explaining individual behavior.

In many respects Mitchell's (1979) arguments can be treated as an adaptation of these views, recognizing the specific features of environmental groups; that is, he has observed that it is possible to find public goods a major motivating force underlying collective behavior. Indeed, based on a survey of the members of five major environmental and conservation groups (i.e., the Environmental Defense Fund, Environmental Action, National Wildlife Federation, Sierra Club, and The Wilderness Society), he found that selective inducements have little bearing on individuals' decisions to join these organizations. Rather, the objectives specified as being important reasons for membership were largely associated with each individual's perception of a public good that he or she realized as a result of the group's actions. Mitchell's survey respondents were asked to rate items that identify their motivations for joining each group according to their importance on a five-point scale and then to rank the three most important reasons. The majority of the respondents in each of the five organizations sampled identified public good motivations as important reasons for belonging to the group. Table 1 summarizes some of the Mitchell findings. It reports by group the percentage of respondents identifying as important those motivations that Mitchell considered related to the public-good outputs of these groups. These concerns were uniformly more important to members than the private good

outputs of the respective organizations. The most significant of the missing elements in Olson's model, which prevents it from dealing with these cases, is the failure to distinguish responses according to the character of the public good or service provided by these groups. In some respects (as I discuss below), his proposed amendment parallels Hansmann's treatment of the features of goods and services that make them more suitable for being provided by nonprofit organizations.

Three considerations influence an individual's membership decision in Mitchell's framework—the cost of joining, the benefits of joining, and *the costs of not joining*. The first two of these motives are, of course, recognized in Olson's analysis. The

new element in Mitchell's framework is the recognition that the features of some goods may well lead the costs of not joining the group to be a strong motivation. Or, equivalently as Mitchell observed:

Drawing on qualitative evidence, including the groups' carefully pretested mail appeals, I argue that under certain conditions a utility for environmental goods will motivate member contributions and that these contributions are compatible with behavior of the egoistical, rational, utility maximizing kind because the cost is low, the potential cost of not contributing [or joining] is high and the individual has imperfect information about the effectiveness of his or her con-

Table 1. Mitchell Survey Findings on Motivations for Joining Environmental Groups^a

Motivation ^b	The Wilderness %	Environmental Defense Fund %	Sierra Club %	Environmental Action %	National Wildlife Federation %
If I don't act now to preserve the environment, things will get much worse.	68	68	64	43	57
If the (<i>relevant organization</i>) achieves its goals, my life and my children's lives will directly benefit.	53	50	56	50	55
My contribution to the (<i>relevant organization</i>) is helping to influence government action on conservation/environmental problems.	43	58	42	—	—
Solving environmental problems is so important that I try to support any effort aimed at that goal.	39	46	30	43	—
N	623	630	661	705	509

^aThese results report the percentage of respondents who rated the statement given as an important motivation to their joining the group which was the highest rating on a five-point scale. These results are summarized from Table 15 in Mitchell (1979).

^bThe remaining motivations identified for respondents to explain their membership in these groups were:

1. I enjoy the (*relevant organization's*) magazine and/or other benefits of membership, such as outings, very much.
2. Some aspects of my life are threatened by environmental or conservation problems.
3. As a result of the encouragement of my friends.
4. I personally gain much from the information I receive from the (*relevant organization*).
5. Many knowledgeable and influential people support the (*relevant organization*).
6. Without contributions like mine, the (*relevant organization*) would be unable to work for improved environmental quality.
7. If I dropped my membership in the (*relevant organization*), I would find it difficult to live with myself.
8. Belonging to the (*relevant organization*) makes me feel really good about what I am doing with my life.
9. My spouse belongs.

tribution in obtaining the good or preventing the bad. (Mitchell, 1979, p. 100, bracketed term added.)

An alternative interpretation of the behavior of environmental groups can be derived using models of the behavior of nonprofit institutions. Two of these efforts seem to have the most potential for transfer to the study of the behavior of environmental groups. The first is Weisbrod's (1977) disequilibrium model for the emergence of nonprofit institutions, and the second is Hansmann's theory based on contract failure.

Consider the Weisbrod framework. The public sector (either national or local) is assumed to provide some level of a collective good to all individuals within its jurisdiction. The quantity and quality provided of this good is assumed to be the result of a political voting process. Further, the model maintains that the individuals served have different utility functions and therefore do not value the public good equally. Although Weisbrod acknowledged the possibility of a variety of financial systems for paying the costs of providing this good, he argued that nearly all of them do not permit every consumer to equate the share of the tax (and thus the implicit price) that he or she pays for these services to the marginal benefit received from them (i.e., it precludes use of the Lindahl prices). To illustrate the difficulties this separation of quantity and price decisions poses, we need only consider a simple analysis of different individuals' demands for the public good. For example, suppose that Figure 1 describes three individuals' demand functions for public good, Q . To keep the analysis simple, assume each individual pays an equal contribution to the cost of the public good and that contribution (or price) is OT . All individuals receive the same quantity of the public good, OQ , by definition. As the figure illustrates, I have assumed that individuals are heterogeneous. Therefore each individual will respond differently to the quantity of the public good provided and to the perceived price paid. To understand these reactions, consider the second individual's demand, as given by D_2 ; this individual finds the government's level of provision and his perceived price completely consistent with his willingness to pay. Thus, he (or she) would not want to change either. The same judgment cannot, of course, be applied for individuals one and three.

For individual one the level of the good provided exceeds his demand at any price (including zero) and the perceived price would lead him to demand none of the good under private market provision. By contrast, individual three is undersatisfied. At a perceived price of OT , this individual would demand RV additional units of the

good. Thus, for both of these individuals, an exogenous determination of the quantity and price has led to dissatisfaction (for different reasons).

Weisbrod's explanation for the existence of nonprofit institutions considers them to be the result of the attempts by those individuals with excess demand for specific public goods to adjust the available level of public goods (or substitutes for them) in order to bring their desires into balance with the level of the good or service provided. Of course, these adjustments are assumed to take place within a framework that recognizes the incremental costs of providing alternatives. More specifically, his "output hypothesis" proposed that:

a class of voluntary organizations will come into existence as *extragovernmental providers of collective consumption goods*. These organizations will "supplement" the *public* provision (which can be zero) and provide an alternative to the *private-sector* provision of the private-good substitutes for collective goods. . . . The amount of collective good provision by the voluntary sector as compared with the public sector depends on the degree to which the public sector is able to satisfy the diverse demands of its constituents. (Weisbrod, 1977, pp. 59-61)

Unfortunately, this hypothesis did not explain the emergence of nonprofit over for-profit organizations for those collective goods that do permit selective exclusion. It also does not seem to deal with those activities by pressure groups seeking to influence the governmentally supplied level of the public good. The first of these limitations is addressed in Hansmann's analysis of nonprofit organizations. He argued that:

Nonprofits tend to produce particular services, those characterized by "contract failure," because consumers prefer to deal with nonprofits in purchasing those services. This preference . . . is based upon a feeling that nonprofits can be trusted not to exploit the advantage over the consumer from contract failure. This trust derives its rational basis from the nondistribution constraint that characterizes the nonprofit form. (Hansmann, 1980, p. 896).

In Hansmann's framework the collective or public nature of the good is only one of the reasons for "contract failure." He identified four others, including cases with:

- 1) a separation between the purchaser and recipient of a private service that makes it difficult for the purchaser to have assurance that the service was provided at the level and quality purchased;
- 2) goods where fixed costs account for a substantial fraction of costs of operation; there are in-

Figure 1.

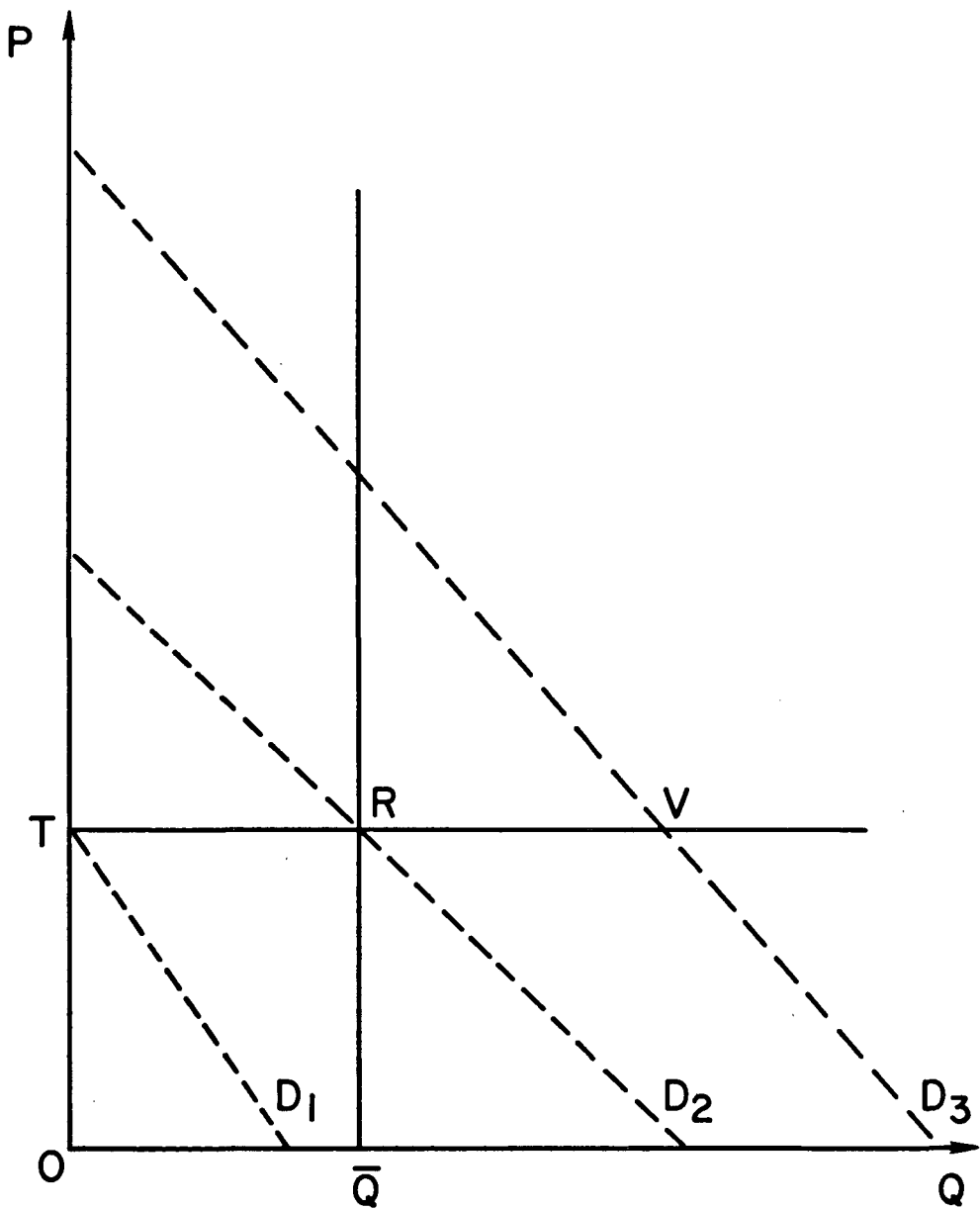


Figure 1

divisibilities in the production process; and the market for the good or service may be so limited that there is generally excess supply (good examples can be found in most of the performing arts). In these cases Hansmann argued that voluntary price discrimination through individual donations is possible. However, this is only true because the nonprofit form is regarded as offering a greater assurance that the donations will be used to continue the service;⁷

3) market imperfections limiting access to certain goods or services, such as loans. For example, legal and practical imperfections may limit an individual's access to loans for higher education and training; and

4) the delivery of complex personal services where the buyer must trust the seller. Services are provided at the discretion of the seller, but the buyer has no technical ability to monitor the outputs.

For my purposes none of these three explanations for nonprofit activities (i.e., Mitchell and Moe, Weisbrod, or Hansmann) provides a fully adequate description of the issues involved with an individual's choice to participate in an environmental public interest group, because each considers the decisions of individuals in isolation from the available alternatives.

Consider, for example, the alternatives before the representative individual who I shall assume is not satisfied with the level of a government-provided public good or service. It is sensible, following Weisbrod, to consider the modes of adjustment available to this individual. These adjustments might involve the acquisition of a private good (or goods) that substitute for the public good. Alternatively, the individual may join a group that seeks to provide a substitute public good whose services are confined to the members of the group (the Weisbrod conception of the emergence of nonprofits). Finally, it is also possible for the individual to attempt to influence the level of government provision through membership in a group that lobbies to increase the provision of the public good of interest. However, in this case the membership alone does not insure adjustment in the amount of the government-provided public good. Rather, the outcome will depend on the number of members in the group (as well as other considerations that may be outside the individual's control). The group may increase the public good provided to its members

or, equally plausible, may prevent it from being reduced which, based on Mitchell's cost of not contributing, may be equally important.⁸

In many cases, the individual does not have to rely on just one of these options but can consider undertaking a subset or, indeed, all of them. Of course it should also be acknowledged that the last of these alternatives is a simple caricature of the environmental organizations available in the real world. They have different histories and constituencies. Some trace their origins to the turn of the century (e.g., the Sierra Club in 1892 and the National Audubon Society in 1905), whereas others arose as litigators in the late sixties and early seventies. Of the 13 environmental groups considered by Mitchell, all had some form of magazine, but only one had an operational outings program. Their orientations toward political activity ranged from the fairly conservative stance of the National Wildlife Federation to the more activist orientation of the Sierra Club. These groups also span other dimensions, including special regional or issue-related interests such as the Defenders of Wildlife, whose focus is on protection of endangered species. Thus not all environmental groups can be expected to fit within the confines of a single type of group. It is important, therefore, to consider the potential role of different types of groups in providing an efficient response to individuals' demands for collective goods. This is clearly consistent with what we observe in practice and with past efforts to model group behavior.⁹

⁸Symonds (1982) clearly suggested that the environmental groups were responsible for preventing the relaxation of environmental regulations and a more pro-business policy. He cited three examples of the groups' influence: (a) the extension of the Endangered Species Act for three years without significant changes; (b) the defeat of former Interior Secretary Watt's attempt to permit some oil and gas leasing in wilderness areas; and (c) the National Clean Air Coalition's success (to this date) in preventing major changes in the Clean Air Act.

⁹Indeed, Chamberlin (1974) explicitly called for such efforts as one step in the development of criteria to evaluate group action. He noted that: "Most of the work making use of the theory of collective goods has thus far dealt with the actions of a single group . . . an important next step in applying this theory to the study of political phenomena would be the consideration of the behavior of several groups in competition" (p. 715). My approach is not fully responsive to this suggestion, because I consider the prospects for a coordinated role of different groups in efficiently meeting individuals' demands for collective goods.

A next step, beyond the scope of this study, would involve consideration of how the private actions of groups would relate to this idealized coordinated solution.

⁷For further discussion of this point see Hansmann (1980, pp. 854-859).

Environmental Public Interest Groups as Clubs

Sandler and Tschirhart (1980) define a club as "a voluntary group deriving mutual benefit from sharing one or more of the following: production costs, the members' characteristics, or a good characterized by excludable benefits" (p. 1482). I propose to use the theory of clubs as a modeling framework for describing the ideal conditions of access to environmental groups; that is, based on how these groups respond to individuals' demands, either directly, through environmental services that substitute for a government-provided public good, or indirectly through lobbying, the model describes the ideal levels of their activities (including the size of their memberships). These conditions are derived under the assumption that the resource allocation decisions are intended to structure the terms for access to these institutions, not to assign specific individuals to specific groups.¹⁰ My model should be distinguished from the Hansmann (1981), Holtmann (1983), and James (1983) positive models of the behavior of nonprofit institutions. In these studies, each author sought to model the decision making of nonprofits, rather than the reasons why individuals would join or support these groups. Holtmann (1983), for example, describes nonprofit organizations as a response to situations where market demand is random. In these cases it may be possible to offer the commodities involved under different rationing systems where the quantity demanded exceeds that supplied at the market price. James, by contrast, considered the use of profit-making activities by nonprofit institutions as one means for supporting (i.e., cross-subsidizing) their primary, nonprofit activities.¹¹

The basic structure of my model follows Hill-

man's and Swan's (1983) recent description of participation rules for clubs. There are, however, several important amendments to the framework they utilize: 1) my description of the production activities necessary for the club-produced public good uses a cost function at the individual club level. This specification is therefore independent of the production transformation function ($F(Z, Y)$) that describes the feasible combinations of the society-wide public good and the private numeraire good available to society;¹² 2) the definition of clubs includes a specification of the membership fees or cost-sharing rules. This approach permits the clubs to reflect the non-distribution constraint (i.e., no profit) that is characteristic of these groups;¹³ 3) the public interest club or lobby does not provide a tangible output. It can be interpreted as a mechanism for enhancing one's ability to petition for increases in the society-wide public good. Thus the output of the club is an increase in the likelihood of changing the level of this public good. Two versions of the model are considered. In one, these increments in the public good are enjoyed exclusively by the club members, whereas in the second everyone receives them; and 4) access conditions are used to define the efficient provision of different types of clubs to individuals.¹⁴ These conditions

¹⁰This approach avoids the difficulties associated with formulating models of the production technology that maintain consistency between the microlevel, club-specific description of the production activities and the economywide summary of a set of clubs' activities. The club results discussed recently by Boadway (1980) are incorrect because they fail to treat the micro and aggregate technologies consistently. See Berglas (1981) and Smith (1980) for further details.

¹¹Sandler and Tschirhart (1980) object to this characterization on the grounds that it imposes a second-best constraint on the objective function for a Pareto-optimal allocation of resources. They observed that the presence of this constraint leads to the disparity in findings on the implications of membership heterogeneity for the efficiency of segregation in clubs. Of course, their observations are within a model that allows members to utilize the club good by different amounts. Nonetheless, their overall conclusion would imply differential charges for different individuals based on their respective valuations of the club good. In their judgment the constraint would only be warranted if the monitoring costs were prohibitive. This position fails to recognize the importance of institutional constraints for the description of efficient resource allocation.

¹²In addition I have assumed club members receive equal quantities of the club-produced good and therefore do not have discretion in their intensity of use, as in Oakland's (1972) model. Sandler and Tschirhart (1980, pp. 1487-1488) provide a discussion of the advantages of the Oakland framework. It was not adopted here, because the present model considered three types of sub-

¹⁰See Hillman and Swan (1983, pp. 59-64) for further discussion of this distinction.

¹¹The contract research activities of some of the nonprofit environmental groups might be viewed in an analogous way; that is, some of these groups have undertaken contract-supported environmental research. These activities might be interpreted as secondary to their overall objectives of promoting governmental activities to maintain and enhance environmental quality and to preserve natural environments. Nonetheless, by defraying some of the organizational overhead, these contracts may serve to cross-subsidize their primary activities.

Research contracts should be distinguished from grants designed to support environmental groups' activities. Generally such funding is relatively free of requirements and would be capable of supporting the groups' primary goals. For a discussion of the importance of grants to the development of environmental groups, see Downing and Brady (1981).

can be interpreted either as offering membership access probabilities, based on an *ex ante* Pareto efficient lottery, or as the membership rules that would be implied by an equally weighted Benthamite additive social welfare function.

The model distinguishes four types of good: a society-wide public good, Z , which is available to all individuals regardless of their club membership; a private numeraire good, y ; and two types of substitutes for the society-wide public good—a club-provided public good, x , available only to members, and a private substitute good, w . This configuration was selected in an attempt to reflect the adjustment process described by Weisbrod as providing the motivations for the formation of nonprofit organizations.

It should be acknowledged, however, that by maintaining that each individual can enter one club I implicitly assume individuals make mutually exclusive choices about the organizations they will support. Thus, the portfolio of jointly undertaken activities that the model can consider involves all possible adjustments from the perspective of society as a whole.

Individuals are assumed to have their preferences described with well-behaved (i.e., differentiable, monotonic, quasi-concave) utility functions ($U^i(\cdot)$).¹⁵ The overall population, N , is fixed and the membership conditions describe the resource allocation necessary to offer each member of the population the same lottery with fixed odds of membership in each of three different groups—the club providing an excludable, environmental public good, the lobby intended to influence the level of government-provided public good (i.e., from Z to $Z+A$), and the general public (i.e., those not affiliated with either group). This system provides equal *ex ante* treatment for each

stitutes for the economywide public good. An Oakland club good was largely equivalent to our private good. The more conventional club membership public good provided a better contrast to the private good and to the services provided the "environmental lobby."

¹⁵To simplify the analysis I have assumed identical individuals. If I were to consider the case of nonidentical individuals, then a ranking of individuals according to their respective preferences for each type of club would be necessary. Such a framework would be analogous to the approach Sandler and Tschirhart (1980) used to provide a general definition of the provision and membership conditions. However, in their case, the ranking was based on preference for the club good. With three states of assignment of individuals, it is not clear any consistent ranking could be derived. Thus, there might be the need to consider a variety of permutations of individuals to describe the Pareto efficient criteria. The assumption of identical individuals eliminates this problem.

member of the population in the sense that no other lottery could improve the expected utility of some member of the population without decreasing the expected utility of some other member. From an individual's perspective it is clear that one either joins or does not join a group. Individuals do not select lotteries. However, when we consider how to coordinate efficiently the activities of a set of groups as if they could be planned to realize an efficient allocation of resources, then the lottery concept provides one means of describing the conditions of access to each group that would be implied by an efficient plan. As Hillman and Swan explain, both this *ex ante* interpretation and an *ex post* explanation of the criteria as the result of selecting membership conditions to maximize social welfare are possible. This follows from the linearity of the specified social welfare function (see Ulph, 1982, pp. 267-268). In what follows I adopt the first of these interpretations. Regardless of their membership condition, each individual is assumed to have access to a certain allocation of the society-wide public good, Z . In addition, he is assumed to be able to "purchase" the private substitute good, w , at a fixed price (in terms of the numeraire good, y) of r .

Equation (1) states the objective function for describing the Pareto optimal allocation.

$$L = NEU + \gamma F(Z, Y) + \theta [Y - A - N_1 y_i - N_2 \bar{y}_i - (N - N_1 - N_2) \bar{y}_i] \quad (1)$$

where:

$$\begin{aligned} EU = & \frac{N_1}{N} U^i(y_i - \frac{c(x, N_1)}{N_1} - r w_i, h^i(x, Z, w_i)) \\ & + \frac{N_2}{N} [P(N_2) U^i(\bar{y}_i - \frac{C(N_2)}{N_2} - r w_i, \\ & h^i(O, Z + A, w_i)) + (1 - P(N_2)) U^i \\ & (\bar{y}_i - \frac{C(N_2)}{N_2} - r w_i, h^i(O, Z, w_i))] \\ & + \frac{(N - N_1 - N_2)}{N} U^i(\bar{y}_i - r w_i, h^i(O, Z, w_i)). \end{aligned}$$

EU describes the expected utility to be realized by the representative individual if society selects a resource allocation that provides N_1/N odds of being a member of a club providing the excludable public good, x , N_2/N odds of being a member of the environmental lobby, and $(N - N_1 - N_2)/N$ odds of being a member of neither. Within each group I have also defined the rules governing the

payments for membership and the corresponding production of each group's services.

The functions $c(x, N_1)$ and $C(N_2)$ describe the costs of operating each of the two kinds of clubs. The first kind produces a public good substitute for Z . The specification selected for the cost function is consistent with many other formulations of the club model at a decentralized level. (See McGuire, 1974, and Berglas, 1976, as examples.) Economic analyses of the club membership and collective good provision decisions have provided two different descriptions of the technologies used by clubs for these activities. When concern has been with the number and types of clubs, the description has generally used a production transformation function to describe all of their activities together (with the number of clubs included as an argument in this function). By contrast, when the focus is on the activities of a predefined number of clubs, the technology is often described using individual cost (or production) functions for each club. There has been some controversy over the consistency between the two descriptions in the recent literature.¹⁶ These concerns need not interfere with our analysis because a microlevel description is necessary to describe the specific institutional features of each group. That is, the cost-sharing arrangement is specifically imposed in each case as a vehicle for reflecting the importance of the nondistribution constraint.¹⁷

The three substitute goods are assumed to enter each individual's utility function within a weakly separable subfunction, $h^i(\cdot)$. This formulation is one way of maintaining that the three goods play a comparable role in contributing to individual utility. They do not exhibit equal marginal utilities, but do bear comparable associations with other goods that enter the individual's utility function. Moreover, it is consistent with the assumption of budget decomposability that im-

plicitly underlies Weisbrod's discussion of non-profit institutions. My analysis will consider differences in the treatment of club members. With the first club, I assume that only the N_1 members of the club receive x . All individuals have access to the society-wide public good, Z , and can purchase the private good substitute, w_i .

The second club does not provide a certain product (or service) to its members. Rather, it offers some prospect of enhancing the level of the society-wide public good they receive by a fixed amount, A . This increase comes at a cost born by society as a whole in terms of a reduced total amount of the private numeraire good, Y , for all. Although the numeraire good is always reduced by A , it is not clear the allocation of A will come to the club members. Thus, there is a probability, $P(N_2)$ of receiving the good.¹⁸ This is bounded between zero and one, and a concave, monotonic, increasing function of N_2 , the number of club members. I consider two formulations of our model—one in which only club members receive A and another in which all of society receives A .

As I noted earlier, $F(Z, Y)$ describes society's transformation function for the private numeraire good in total and the society-wide public good. A tilde ($\sim$) and a bar ($\bar{\cdot}$) are used for notational convenience to identify individual utility levels by club type and to specify individual selections of y_i . This format serves to highlight that individuals in each club may not receive the same allocations of y . γ and θ are all Lagrangian multipliers.

If I consider the first interpretation of the lobby, differentiate equation (1) with respect to x , y_i , w_i , N_1 and N_2 , and rearrange the resulting first-order conditions, I can specify provision conditions for x , w_i , and Z as well as conditions for the access terms for each of the two types of clubs. These are presented in equations (2) through (6), with MRS designating the marginal rate of substitution, and $EMRS$ the expected marginal rate of substitution. That is:

$$MRS_{xy}^i = \frac{\left(\frac{\partial U^i}{\partial x}\right)}{\left(\frac{\partial U^i}{\partial y_i}\right)}$$

¹⁶Further discussion of this issue is available in Berglas (1981) and Smith (1980).

¹⁷Sandler and Tschirhart (1980) have characterized such constraints as converting the Pareto-optimal club framework to a second-best format. This characterization is somewhat misleading. Although it is true that replacing either of these constraints with one reflecting an individual's specific costs to the club (under the assumption of varying intensities of use) can lead to higher levels of welfare for some individuals without reducing the utilities of others, it fails to recognize that each specification for the payment constraint describes an alternative institutional framework. Institutions limit the feasible allocations of goods and services and should be reflected in the description of the conditions of the politically feasible, economically efficient resource allocations. The use of these cost-sharing constraints in this model offers one simple means of characterizing the nondistribution constraint of nonprofit organizations.

¹⁸This is somewhat similar to a framework adopted by Austen-Smith (1981) in describing an individual's motives for joining a pressure group. In his framework, membership can serve to add a constant amount to the individual's utility function; that is, if the group is successful, the individual utility is $U_i + A$ and if not successful, it is U_i . His analysis is in terms of a "within club" framework and considers an individual's allocation of time to club activities in the presence of uncertainty as to the success of the group's activities.

$$\text{and } EMRS_{wy}^i = P(N_2) \frac{\frac{\partial U^i}{\partial w_i}}{\frac{\partial U^i}{\partial \tilde{y}_i}} + (1 - P(N_2)) \frac{\frac{\partial U^i}{\partial w_i}}{\frac{\partial U^i}{\partial \tilde{y}_i}}$$

$$\sum_{i=1}^{N_1} MRS_{xy}^i = N_1 MRS_{xy} = \frac{\partial c}{\partial x} \quad (2)$$

(provision of x , because all individuals are assumed identical)

$$MRS_{wy}^i = EMRS_{wy}^i = r \text{ for } i = 1, 2, \dots, N. \quad (3)$$

(provision of w)

$$N_1 MRS_{Zy_i}^i + N_2 EMRS_{Z\tilde{y}_i}^i + (N - N_1 - N_2) MRS_{Z\tilde{y}_i}^i = MRT_{ZY}.$$

(provision of Z)

$$\frac{U^i}{\partial y_i} - \frac{\bar{U}^i}{\partial \bar{y}_i} + (\bar{y}_i - y_i) = \frac{\partial c}{\partial N_1} - \frac{c(x, N_1)}{N_1} \quad (5)$$

(membership in the first club—the environmental group)

$$\frac{E(\bar{U}^i)}{E(\frac{\partial \bar{U}^i}{\partial \bar{y}_i})} - \frac{\bar{U}^i}{\partial \bar{y}_i} + (\bar{y}_i - \tilde{y}_i) + N_2 \frac{dP}{dN_2} \frac{(U^i(\cdot, Z+A) - U^i(\cdot, Z))}{E(\frac{\partial \bar{U}^i}{\partial \bar{y}_i})} = \frac{dC}{dN_2} - \frac{C(N_2)}{N_2} \quad (6)$$

(membership in the second club—the environmental lobby)

where $\tilde{U}^i(\cdot, Z+A)$ is a shorthand expression for

$$U^i(y_i - \frac{C(N_2)}{N_2} - rw_i, h^i(O, Z+A, w_i))$$

and $\tilde{U}^i(\cdot, Z)$ corresponds to

$$U^i(y_i - \frac{C(N_2)}{N_2} - rw_i, h^i(O, Z, w_i)).$$

To simplify these expressions I have adopted some notation conventions. First, a bar ($\bar{\cdot}$) or tilda ($\tilde{\cdot}$) over y_i or U^i is used throughout to designate the individual's status (i.e., no bar or tilda indicates a member of the first club, a tilda indicates membership in the lobby, and a bar membership in neither).

The most interesting aspect of the model's insights arises with the membership conditions. Equations (2) through (4) provide the conditions for optimal provision levels of the club public good (x), the private substitute (w), and the society-wide public good (Z), respectively. Equation (2) indicates that the level of x provided should be such that the aggregate marginal benefit for club members ($N_1 MRS_{xy}$) equals the marginal cost of producing x (measured in units of the private numeraire— y). This is the standard result from club theory (see Berglas, 1976; Buchanan, 1962; and Sandler & Tschirhart, 1980, as examples). Equation (3) indicates that individuals, regardless of their membership in one of the clubs, will acquire the private good, which functions as a substitute for the public goods (i.e., Z and x) until the marginal benefit (or willingness to pay) measured in terms of the numeraire y equals the marginal cost (also in terms of y). This arises because each individual faces the same access condition for the clubs (and the price of w_i is constant across membership states), the "certain" marginal benefits from w for members of the first club or nonmembers or both will equal the expected marginal benefit realized by members in the lobby.

In equation (4) there is the now familiar Samuelsonian condition for the level of provision of the economywide public good, Z ; that is, the individual demands for collective good are summed vertically, and the result is equal to the marginal cost of providing the public good (MRT_{ZY}) in order for an efficient level of Z . My formulation implicitly accepts the Diamond (1967) and Sandmo (1972) approaches to defining *ex ante* Pareto efficient allocations, and in this case treats expected and actual marginal rates of substitution in equivalent terms, which means that society is adopting a risk-neutral attitude. That is, equation (4) specifies an efficiency rule that treats the expected marginal rates of substitution (which are comparable to inverse demands functions) as the certainty equivalent demands.

My membership conditions are quite different from all earlier discussions of club membership rules, including Sandler and Tschirhart (1980), Hillman and Swan (1983), and earlier work by Helpman and Hillman (1977). The differences arise from two sources: the prospects for membership in either of two clubs, each with a different type of service to offer; and an explicit intro-

duction of the institutional arrangements governing each individual's membership fee. The access conditions for these clubs use the nonmembership state as a benchmark. Thus, the benefit (from society's perspective) to increasing the *ex ante* prospects for membership in the first club is the incremental utility as a result of membership (measured in terms of y) plus the change in the allocation of the private numeraire good required to assure that

$$\frac{\partial U^I}{\partial y_i} = E\left(\frac{\partial U^I}{\partial \tilde{y}_i}\right) = \frac{\partial U^I}{\partial \bar{y}_i}.^{19}$$

Given our predefined payment scheme, the cost to society is determined by the cost imposed on each

club by an additional member (i.e., $\frac{dC}{dN_2}$ or $\frac{\partial c}{\partial N_1}$)

in comparison to the membership fee or average cost. In effect, social efficiency requires I judge the costs of providing membership according to whether additional members would pay for the costs they impose on the group. It is important to note that I do not require that they pay the full incremental costs of their membership. Rather, efficient access conditions recognize that the benefits these members realize directly or, in the case of the lottery, confer on others indirectly, must also be considered.

The environmental lobby (i.e., the second club) includes two types of benefits associated with membership: the incremental utility measured as the expected utility from lobbying activities, and the incremental benefits associated with each member's contribution to the probability of successful lobbying activity. In other words, because the probability of realizing an increment to Z from lobbying was expressed as a function of the size of the lobby, each additional member raises the likelihood of success, and all existing members gain as a result. Their gain is the incremental utility provided by an increase from Z to $Z + A$ weighted by the increased likelihood of success as

a result of the additional member, $\frac{dP}{dN_2}$. These in-

cremental benefits, including the external benefits provided by a new member to all existing members, are equated to the excess of marginal operating costs over the membership fee (all measured in terms of the numeraire good y).

This description of the lobby implicitly main-

tains that there is a mechanism to assure that the increment (A) to the public good (Z) as a result of lobbying is realized only by members of the lobbying group. If I drop this assumption, the benefits of lobbying activity are spread more widely; that is, additional terms reflecting the increment to utility (measured in units of y) for all individuals would be counted among the benefits. Of course, these are present in this description because we are examining a socially optimal description of the club access conditions. In a description of the private incentives for lobbying activities, these would be externalities (and one would conclude a sub-optimal level of the activity was undertaken).

How do these results relate to describing an individual's decisions for joining an environmental lobbying group or other nonprofit, public interest group providing goods that substitute for government-provided public goods? They suggest that on efficiency grounds (recognizing the potential role of institutional constraints), these activities should be undertaken. Thus, the private actions we observe in supporting environmental lobbies can be explained in a rational economic framework. In addition, they indicate that with some modification rational behavior need not preclude collective action. However, it should be acknowledged that in both of the clubs described here, some form of exclusion or influence is assumed to be present. For the group providing x , the club is able to restrict services to those individuals joining the club. By contrast with the environmental lobby, the ability to influence the provision of Z is assumed to reside with the lobby. This influence is similar to self-protection activities discussed in the Ehrlich-Becker (1972) analysis of insurance. My analysis of this case suggests that exclusion is not essential, but the ability to exercise either exclusion or influence is. Of course, it should be acknowledged that without real or perceived exclusion, the incentives for an individual to join the lobby would be greatly diminished. They may not be zero. As the marginal conditions suggest (considering only the terms associated with one individual), the incentives depend on the quality of the available substitutes (i.e., x and w_i), and the respective costs of each to the individual.

Any judgment about the importance of these modifications to Olson's framework depends on whether they are plausible descriptions of why individuals join the groups. Does an individual's residential location or some other aspect of the constraints to his (or her) allocation decisions (such as ability to take vacation time) permit him to take greater advantage of increments to the society-wide public good? Is membership in a lobby the most cost-effective way of assuring an increase in the supply of a public good regardless of whether the increase is also made available to

¹⁹See Hillman and Swan (1983, pp. 61-63) for a discussion of the implications of this condition in models where club participation is uncertain and a parallel requirement when it is certain.

others? This possibility offers an alternative to Moe's argument that some collective goods may actually generate their own selective incentives because of the unique role of purposive incentives (i.e., benefits derived from the support and pursuits of worthwhile collective goods). In other words, we do not need to assume some form of altruism to explain membership in environmental lobbies.

Finally, it is also possible to argue (within a model that allows for heterogeneous individuals) that some individuals' valuation of environmental services may not be exclusively associated with their use of these services. There has been increasing recognition of the importance of existence values in the aggregate benefits associated with unique natural environments (see Schulze, Brookshire, Walther, & Kelley (1981)).²⁰ Consequently, one might also interpret the membership in environmental lobbies as composed of individuals who realize existence values in excess of that associated with the direct, user-oriented level of the collective good (Z). In such a framework, the increment (A) might be interpreted as a reflection of these nonuser services.

These efficiency conditions also suggest that several types of groups can exist simultaneously (even under the restrictive assumption of mutually exclusive membership patterns). Moreover, the existence of either society-wide public or private goods that substitute for the services provided by the club does not, in itself, preclude the existence of an environmental public interest group as an institutional mechanism for improving the allocation of resources. An important distinction between an *ex ante* Pareto-optimal formulation of the role of environmental groups and one based on a positive model of individual decision making arises with the character of the information available to the individual. For example, both the Mitchell (1979) and the Moe (1980) explanations of the economic rationale for an individual joining a public interest group rely on an assumption of incomplete information. In Mitchell's case this was associated with a rather large discrepancy between $\bar{U}^i(\cdot, Z + A)$ and $\bar{U}^i(\cdot, Z)$, as well as the perception that each individual's membership can influence the realization of the group's objec-

tives (i.e., $\frac{dP}{dN_2}$ is perceived as significantly different from zero). My analysis maintained that these relationships are known and constrain the *ex ante* resource allocation decisions. In the real world, one can expect a diversity of information and perceptions; this may further enhance the prospects for actual membership decisions.

Voluntary Price Discrimination and Clubs

The model described in the previous section ignores one important aspect of individuals' relationships to nonprofit groups. In many cases, individuals do not simply join the group and pay their membership fee. There can be substantial voluntary contributions of time and monetary resources in support of these groups.²¹ Hansmann (1981) describes these responses as a form of voluntary price discrimination made possible because individuals trust the organization to use donations to support the goods or services provided. For some environmental groups, donations can be an important component of their income. Mitchell (1979) reports, based on personal interviews with the membership directors of environmental groups, that between 17 and 49% of the members of the five groups with available data made contributions above the ordinary membership fee.²² Nonetheless, it is difficult to gauge directly the magnitude of donations as a fraction of any group's total income. Mitchell's (1980) subsequent analysis of the five other environmental groups (including three of the groups reporting the proportion of the membership donating in excess of dues) reported the membership fee and income from members (including the fee and member donations). Table 2 uses this information together with current membership levels to calculate an approximate estimate of the total donations. These crude estimates are likely to understate actual donations. Nonetheless, they indicate donations are an appreciable component of each group's operating budget, ranging from \$70,000

²¹This was the primary focus of Austen-Smith's (1981) model.

²²Mitchell (1979) also reports information on the percentage of the membership of selected environmental groups that made contributions higher than the basic membership fee. His information was based on personal interviews with the membership directors of several of the organizations. They include three of the groups reported in Table 2: Environmental Defense Fund, 32.0%; Sierra Club, 2.4%; Wilderness Society, 17.0%.

For the Sierra Club he reported the percentage of the membership donating \$50 or more in addition to paying their dues. See Mitchell (1979, pp. 107-110) for further discussion.

²⁰Existence value arises when an individual gains utility from the presence of a good, generally a unique natural endowment, even though there is no direct consumption of its services. The theoretical implications of this concept have not been fully developed in the literature. Some economists have described them as a form of either vicarious consumption or of stewardship motives for the environmental resources.

per year for the Sierra Club (which is substantially less than the donations reported by Downing & Brady, 1981) to \$5.2 million for the National Wildlife Federation. Thus, it seems reasonable to consider the role of donations in an *ex ante* Pareto framework.

I will consider the case where donations are made to the environmental lobby group; moreover, I will assume that aggregate donations increase the probability of realizing the predefined increment to the economywide public good (A). This specification implies that $P(N_2)$ in equation

(1) would be replaced by $\bar{P}(N_2, D)$ where $D = \sum_{i=1}^{N_2} d_i$

with d_i representing each individual's donation expressed in terms of y_i^j . Of course, each individual's y_i would also be reduced by d_i to reflect the voluntary contribution to support the club in excess of the constant membership fee— $C(N_2)/N_2$. Within the *ex ante* Pareto decision-making framework each individual would be assumed to donate to the club until his expected loss from the contribution equaled the aggregate expected improvement for all members resulting from the effects of the aggregate donations on $\bar{P}(n, D)$.²³

²³If I assumed that the increment A was received by everyone regardless of whether or not they were members of the lobby, I simply add terms to the left side of equation (7) reflecting the utility increments for these groups and their membership levels.

$$\frac{N_2 \frac{\partial \bar{P}}{\partial D} (\bar{U}^i(\cdot, Z+A) - \bar{U}^i(\cdot, Z))}{E\left(\frac{\partial \bar{U}^i}{\partial y_i}\right)} = 1. \quad (7)$$

In addition, in equation (6) the change in the private good to equalize marginal utilities of the numeraire good across alternative states will include an adjustment for an individual's donations to the lobby (i.e., $\bar{y}_i - \tilde{y}_i$ will be replaced by $\bar{y}_i - \tilde{y}_i - d_i$).

The marginal cost of a donation is one, because donations are expressed in terms of y , and each unit donated is assumed to be completely allocated to lobbying activities. If, by contrast, I assumed a fraction of total donations, say α ($0 < \alpha < 1$) would be allocated to lobbying activities, then the expected incremental gains (to society) in equation (7) would equal $1/\alpha$.

Of course, what is important from the perspective of explaining membership decisions is how the individual will perceive his or her benefit resulting from a donation. The Pareto criterion internalizes the expected increment to all members' utilities resulting from an increase in the societywide public good to $Z + A$. To the extent an individual's perception is limited to personal (or perhaps family members') utility increments, donations will be less than the efficient levels. However, they will not necessarily be zero. This is Hansmann's point in discussing the prospects for voluntary price discrimination.

Table 2. A Comparison of Membership Fees and Total Contributions^a

	Membership Fee	Average Amount Received from Members	Membership in 1982 ^b (millions)	Estimated Annual Donations (millions)
National Wildlife Federation ^c	\$ 7.50	\$16.24	4.20	\$5.21
Environmental Defense Fund	15.00	17.56	0.05	0.13
Wilderness Society	15.00	17.36	0.07	0.17
Sierra Club	20.00	20.22	0.31	0.07
Environmental Action	15.00	15.00 ^d	0.03	

^aThese estimates are based on 1978 membership fees reported in Mitchell (1980, p. 50) and contribution information for 1977, 1978, and 1979 reported in Mitchell's Table 10. The calculations assume that the membership fee did not change over the period.

^bThese membership estimates are taken from Symonds (1982).

^cThe distinction between membership fee and the average amount received from members is somewhat misleading in this case. The \$7.50 fee provides membership in the National Wildlife Federation and the *National Wildlife Magazine*. A \$15.00 fee provides membership, the *National Wildlife Magazine*, and the *International Wildlife Magazine*. I have used the higher of these two figures to provide a conservative estimate of the total donations. I am grateful to John Tschirhart for calling this point to my attention.

^dThe estimated average amount received was somewhat less than the membership fee. Presumably this difference was the result of using 1977 income from membership and the 1978 membership fee.

Implications

This article has explored the possibility of using a society-wide Pareto optimal description of club membership and provision conditions to evaluate the role of public interest groups in promoting an efficient allocation of resources. The model used for this purpose extended the *ex ante* Pareto conditions recently presented by Hillman and Swan (1983) by considering the definition of the conditions of access to each of three possible groups. The use of access to membership provided the basis for our *ex ante* analysis of the membership conditions. Two types of clubs were introduced, one providing a public good to its members, and the second (intended to represent the environmental lobby) attempting to influence the level of the society-wide (environmental) public good provided to its membership (as well as more generally). This characterization of the efficient assignment and provision conditions allows the model to consider aspects of the Weisbrod, Mitchell, Moe, and Hansmann analyses of the reasons for and role of nonprofit organizations.

In a genuine sense the model described here attempts to formalize and extend Moe's (1980) general analysis of groups and Mitchell's (1979) more specific evaluation of individuals' membership decisions in environmental groups. The results clearly support a rational explanation of the role of those groups when there exists either a basis for influencing the provision of a public good or providing some substitute excludable good. Indeed, the conditions for an efficient allocation of resources admit the possibility for the existence of different types of clubs, providing goods or services that substitute for or offer the prospect of enhancement in the supply of an economy-wide public good. Thus, the model does not invalidate Olson's general argument, which relies on the inability of groups to exclude nonmembers from enjoying their collective services. Rather, this model recognizes the exclusion is only one of the forms selective incentives can take.

The framework also suggests that donations as a form of voluntary price discrimination can be considered a part of an efficient resource allocation, when these donations influence the probability of success of the environmental lobby. My conditions are also consistent with Hansmann's judgments about what can be expected from donations in a framework structured to describe an individual's choices. In this case, the individual's level of donations will not necessarily be zero; rather it will depend on that individual's perception of the effects of his contribution for the success of the group. This finding is clearly consistent with Moe's (1980) arguments emphasizing the importance of limited information and

bounded rationality in explaining group behavior. It is also compatible with Mitchell's (1979) arguments for membership in environmental groups and Austen-Smith's (1981) evaluation of the allocation of individual effort to pressure groups.

Nonetheless, the model remains quite limited. The characterization of uncertainty used in the outputs of the environmental lobby was rather simple. Individuals were assumed to be homogeneous and to be members of only one club at a time. Finally, I did not attempt to explore the relationship between decentralized models of club behavior and this economy-wide, centralized assignment framework. Each of these limitations suggests possible areas for improvement and extension. Nevertheless, the analysis does suggest that Mitchell's arguments for membership in collective groups as a rational decision can be supported and extended within a generalized club-theoretic framework.

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Provision of Public Goods and the MCS Experimental Paradigm

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Van de Kragt, Orbell, and Dawes (1983) reported the results of a series of experiments in which n subjects each receive a monetary endowment and then may choose privately whether to contribute it to a monetary public good; the good is supplied if a prespecified number $m < n$ of contributions or more is made. Decision policies maximizing expected value are derived for this experimental paradigm under three different assumptions about the expectations each individual has about the decisions of the other $n - 1$ members: A homogeneity assumption postulating that each other member contributes with fixed probability p , a heterogeneity assumption postulating that the $n - 1$ p 's are independently selected from a subjective probability distribution, and a partial homogeneity assumption postulating that the $n - 1$ members are partitioned into distinct subsets with the same p for all members of each subset. The theoretical and social implications of these assumptions are briefly discussed.

Public goods are defined by two properties, namely, jointness of supply and impossibility of exclusion (Barry & Hardin, 1982). A basic prediction concerning public goods, derived from a straightforward extension of the standard economic model of rational choice, is that they will be underprovided relative to demand or, in the extreme case, will not be provided at all. "Rational and selfish individuals will recognize the opportunity to 'free ride' on the contributions of others which public goods offer and, secure in the knowledge that they can share in the good once it is provided, will withhold or severely curtail their own contributions" (van de Kragt, Orbell, & Dawes, 1983, p. 112). To explain the fact that people demonstrably do contribute toward public goods, it has been natural to replace the model of narrow rationality defined for the case of self-interested motivation by a more general model that prescribes maximization of expected utility in which the utility is a function of the payoffs (contributions and benefits) associated with each decision as well as other individual and social consequences (e.g., satisfying one's conscience, contribution to the welfare of others).

Apparently motivated by the work of Campbell (1975) and Hardin (1968, 1977), who have viewed

with skepticism or mistrust solutions to social problems founded on altruism, conscience, and social norms as explanatory constructs, van de Kragt et al. posed the following question: "Under what circumstances will individuals who are *not* constrained by a concern for the welfare of others nevertheless act so as to provide valued public goods for the groups of which they are members?" (1983, p. 113). Rather than attempting to answer this question by theoretical arguments, van de Kragt et al. proposed and employed a simple and attractive experimental paradigm which, if further extended, may successfully compete with the familiar n -person Prisoner's Dilemma paradigm. They reported the results of a series of small group experiments in which seven subjects each receive a fixed monetary endowment and then may choose privately whether to contribute the endowment to a monetary public good, and in which the good is supplied if a prespecified number of contributions or more are made. Communication among the subjects was the major independent variable in these experiments. Given the opportunity to discuss the problem before making their decisions privately, the subjects organized themselves by specifying the contributors in a way that always resulted in an efficient provision of the public good. But even when communication was prohibited and feedback was not given, the public good was provided 65% of the time, and when provided, inefficiency resulted more than 50% of the time because of over-provision.

My concern in the present article is with the traditional, no-communication, single-trial step-good collective action with binary contributions (Hardin, 1982). If the benefit to the group increases as the number of contributors grows, with

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no minimal contributing set (MCS), the provision of public goods may be modeled as an n -person Prisoner's Dilemma game in which the (pure) strategy not to contribute is unconditionally best (Hardin, 1971, 1982). But if, as in the experimental paradigm above, a MCS is required for the public good to be provided, an unconditionally best strategy can be no longer identified (Frohlich & Oppenheimer, 1978; Hardin, 1982). For an expected value maximizer, the decision to contribute depends on the size of the group, the size of the MCS, the values of the endowment and the benefit, and the probability that the decision maker assigns to the event that each of the other members of his or her group will contribute. Van de Kragt et al. did not attempt to derive decision policies maximizing expected value and apply them to the particular parameters used in their MCS study. Therefore, the answer to the question they originally posed is incomplete, and although Frohlich and Oppenheimer (1978, chap. 3) and Hardin (1982, chap. 4) undertook an expected value maximization analysis of the MCS paradigm, they left the decision maker's beliefs or expectations regarding the decisions of the other group members completely unspecified. Following Straffin's (1978) probabilistic approach to the two classical power indexes of Shapley and Shubik (1954) and Banzhaf (1965), the present article proposes three alternative assumptions about the decision maker's beliefs and then examines briefly their social implications.

Because I am primarily interested in developing expected utility models that are experimentally testable, my focus is on small groups. As the group size grows, the MCS paradigm approaches the n -person Prisoner's Dilemma game. Consequently, the results derived below from the three different models of the decision maker's belief structure become progressively weaker and of less interest as the number of group members grows. In this respect, my results agree with Olson's (1965) central conclusion regarding the effects of group size on the likelihood of group success: large groups will fail, small groups may succeed.

The MCS Experimental Paradigm

The single-trial MCS experimental paradigm described by van de Kragt et al., in which pre-decision communication among group members is prohibited, may be characterized by the following assumptions:

1. The game is played by a group $N = \{1, \dots, n\}$ of n players who are unfamiliar with one another. Communication before or during the game is prohibited.
2. Each player i ($i \in N$) receives a fixed monetary endowment of e units ($e > 0$), which is his or hers to keep.
3. Having received the endowment, each player i must decide privately whether or not to contribute it to the group's benefit.
4. If m or more players contribute, each player i receives a reward (public good) of r units. If $m - 1$ or fewer players contribute, each of the contributors loses his or her endowment.
5. With the exception of the final payoff, no feedback is provided about the game. In particular, player i never learns how many players actually contributed.

I assume that $r > e$. Assumption (5) places mild restrictions on n and m . The first and obvious restriction is $n > 3$. Another restriction is $1 < m < n - 1$. Note that if $m = 1$ and the payoff of player i is 0, player i learns that nobody else contributed, contrary to assumption (5). Similarly, if $m = n - 1$ a payoff of $r + e$ tells player i that all other $n - 1$ players contributed, contrary to assumption (5).

The payoff matrix for player i is shown in Table 1. Let P_{m-1} denote player i 's probability before making his or her private decision that exactly $m - 1$ other players will contribute, and let P_{m+} denote player i 's probability that m or more other players will contribute. Then the expected value of contributing for player i is

$$EV(C) = r(P_{m-1} + P_{m+}),$$

and the expected value of not contributing is

$$\begin{aligned} EV(\bar{C}) &= e(1 - P_{m+}) + (e + r)P_{m+} \\ &= e + rP_{m+}. \end{aligned}$$

Defining $D = EV(C) - EV(\bar{C})$, I obtain immediately

$$\begin{aligned} D &= r(P_{m-1} + P_{m+}) - e - rP_{m+} \\ &= rP_{m-1} - e. \end{aligned} \quad (1)$$

Thus, if he or she subscribes to the maximization of expected value, player i should contribute if $rP_{m-1} > e$ and not contribute otherwise. Because P_{m-1} is the probability of player i making a difference for the attainment of the public good, or being *critical*, equation (1) implies that in order to contribute the expected value of making a difference must be greater than the individual's cost of contributing to the public good. Alternatively, the probability of being critical must exceed the

Table 1. Payoff Matrix for the MCS Paradigm with No Communication and Feedback

Player <i>i</i> 's decision	Number of Players Contributing		
	<i>m</i> - 2 or fewer	exactly <i>m</i> - 1	<i>m</i> or more
Contribute (<i>C</i>)	0	<i>r</i>	<i>r</i>
Not Contribute ($\bar{C}$)	<i>e</i>	<i>e</i>	<i>e</i> + <i>r</i>

Note: $r > e > 0$.

ratio e/r of cost to benefit (Frohlich & Oppenheimer, 1978).

Probability Models for the MCS Paradigm

For a rational and self-motivated player who desires to maximize expected value, the decision of whether or not to contribute depends on the expectations he or she has about the behavior of others. To model these expectations, I exploit a distinction made by Straffin (1977, 1978), who proposed an approach to measure voting power as an answer to a probabilistic question about voting outcomes. Straffin's answer is based on probabilistic assumptions about the nature of the issues to be decided by the weighted voting body as well as the degree of cohesiveness among the members of the body. Both Straffin's approach and the approach proposed in the present article are basically intended to answer the same question, namely, what is the probability that player *i*'s decision will make a difference in the outcome? The contexts, however, are quite different. Whereas Straffin is concerned with the probability that a bill which player *i* supports passes, but would fail if player *i* changed his or her vote, or that a bill which player *i* opposes fails, but would pass if player *i* changed his or her vote, the present approach is concerned with the probability that a group will succeed if player *i* decides to contribute, but will fail otherwise.

Let p_j be interpreted then as player *i*'s probability that player *j* ($j \in N, j \neq i$) will contribute. Two possible assumptions are the following:

Homogeneity Assumption. A number p ($0 \leq p \leq 1$) is selected, and $p_j = p$ for all *j*; or

Heterogeneity Assumption. The p_j 's are selected independently from the beta distribution $B(\alpha, \beta)$ on $[0, 1]$.

According to the homogeneity assumption, player *i* does not distinguish among the $n - 1$

remaining anonymous players. Rather, he or she assigns each of them the same probability of contributing. On the other hand, the heterogeneity assumption recognizes individual differences among the $n - 1$ remaining members of the group. The assumption that the p_j 's are independently selected from the beta distribution is not at all restrictive (Raiffa & Schlaifer, 1961); it allows much flexibility in describing player *i*'s expectations regarding the decision of his or her coplayers.

Because P_{m-1} is player *i*'s probability that exactly $m - 1$ of the remaining $n - 1$ players will contribute, the homogeneity assumption yields directly

$$P_{m-1} = \binom{n-1}{m-1} p^{m-1} (1-p)^{n-m}. \quad (2)$$

Substituting equation (2) for P_{m-1} in equation (1) and writing $D(p)$ for D to express the dependency of the difference between the expected values $EV(C)$ and $EV(\bar{C})$ on p , yields

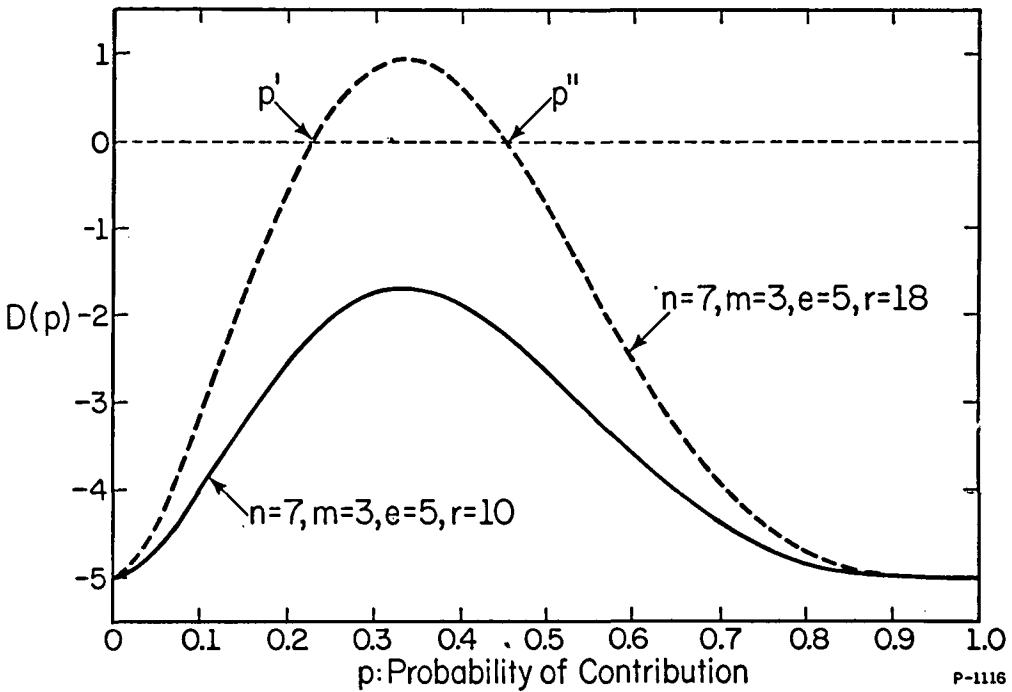
$$D(p) = r \binom{n-1}{m-1} p^{m-1} (1-p)^{n-m} - e. \quad (3)$$

The function $D(p)$ in equation (3) is unimodal. It reaches its minimum of $-e$ when either $p = 0$ or $p = 1$ (see Figure 1) and its maximum at $p^* = (m-1)/(n-1)$. As shown in Figure 1, if a horizontal line is plotted at 0, $D(p)$ will fall below the line, touch the line in a single point, or cross the zero line at two points. It follows that if $D(p^*) \leq 0$, player *i* should never contribute for any value of p . If $D(p^*) > 0$, player *i* should contribute only if $p' \leq p \leq p''$, where p' and p'' ($0 < p' \leq (m-1)/(n-1) \leq p'' < 1$) are the solutions of the equation

$$r \binom{n-1}{m-1} p^{m-1} (1-p)^{n-m} - e = 0.$$

Because the equation above has at most two solutions, the homogeneity assumption prescribes a closed range $[p', p'']$ when $D(p^*) > 0$. A player should not wish to contribute if too

Figure 1. Two Examples of the Expected Value Difference Function $D(p)$ under the Homogeneity Assumption



few others are expected to contribute (thus wasting her contribution) or if too many others are expected to contribute (she might as well take a free ride).

Example. In the experiments reported by van de Kragt et al., $e = \$5$, $r = \$10$, $n = 7$, and either $m = 3$ or $m = 5$. The solid line in Figure 1 depicts the function $D(p)$, which was computed from equation (3) for $e = 5$, $r = 10$, $n = 7$, and $m = 3$. Because $D(p^*) = D(1/3) = -1.708$, player i should never contribute, regardless of her expectations about the behavior of the other players. The function $D(p)$ for $m = 5$ has the same shape as in Figure 1, with p and $1 - p$ reversed, and is therefore not plotted. Because $D(p^*) = D(2/3) = -1.708$ for $m = 5$, player i should again never contribute in this experimental condition.

To show the dependency of the result reported by van de Kragt et al. on the actual parameters they used, suppose that the value of the public good in their experiment is changed from $r = 10$ to $r = 18$, whereas the other parameters remain the same. The broken line in Figure 1 portrays the function $D(p)$ for $e = 5$, $r = 18$, $n = 7$, and $m = 3$. $D(p)$ reaches its maximum at $P^* = 1/3$ as before. However, it now intersects the zero line at two points: $p' =$

0.23 and $p'' = 0.45$. When r is increased from 10 to 18, a maximization of expected value policy dictates to contribute if $0.23 \leq p \leq 0.45$ and not to contribute otherwise.

Under the heterogeneity assumption, a sample of size $n - 1$ is drawn by binomial sampling from a Bernoulli process whose singular parameter is not a fixed constant p but rather a random variable $\tilde{p}$ distributed according to a beta distribution with two parameters $\alpha > 0$ and $\beta > 0$. Then the unconditional distribution of the random variable $\tilde{m}$ is a compound distribution known as the beta-binomial (Raiffa & Schlaifer, 1961) or Pólya-Eggenberger (Johnson & Kotz, 1969) distribution with the probability mass function

$$f_{bb}(m|\alpha, \beta, n-1) = \int_0^1 f_b(m|p, n-1)f_{\beta}(p|\alpha, \beta)dp \\ = \frac{(m+\alpha-1)!(\beta+n-m-2)!(n-1)!(\alpha+\beta-1)!}{m!(\alpha-1)!(n-m-1)!(\beta-1)!(\alpha+\beta+n-2)!} \quad (4)$$

where $n = 1, 2, \dots$; $m = 0, 1, \dots, n-1$; α and β are positive integers.

Computing P_{m-1} from equation (4), substituting it for P_{m-1} in equation (1), and then writing $D(\alpha, \beta)$ for D in equation (1) to express

the dependency of the difference $EV(C) - EV(\bar{C})$ under the heterogeneity assumption on the distribution of $\bar{p}$, yields

$$D(\alpha, \beta)$$
$$= r \left[\frac{(m + \alpha - 2)!(\beta + n - m - 1)!(n - 1)!(\alpha + \beta - 1)!}{(m - 1)!(\alpha - 1)!(n - m)!(\alpha + \beta + n - 2)!} \right]$$

- e. (5)

When the probability distribution of $\bar{p}$ is uniform ($\alpha = \beta = 1$), $f_{\beta b}$ in equation (4) reduces to $1/n$ and equation (5) reduces to $D(1, 1) = r/n - e$.

Example. Suppose that $e = 5$, $r = 10$, and $n = 7$ as in the van de Kragt et al. experiment, and that player i 's expectations regarding the decisions of her co-players are appropriately represented by a beta distribution with parameters $\alpha = 2$ and $\beta = 4$. The beta distribution $B(2, 4)$ is unimodal and negatively skewed with mean $\alpha/(\alpha + \beta) = 0.333$ and standard deviation $(\alpha\beta/[(\alpha + \beta)^2(\alpha + \beta + 1)])^{1/2} = 0.178$. The mean and standard deviation of the number of

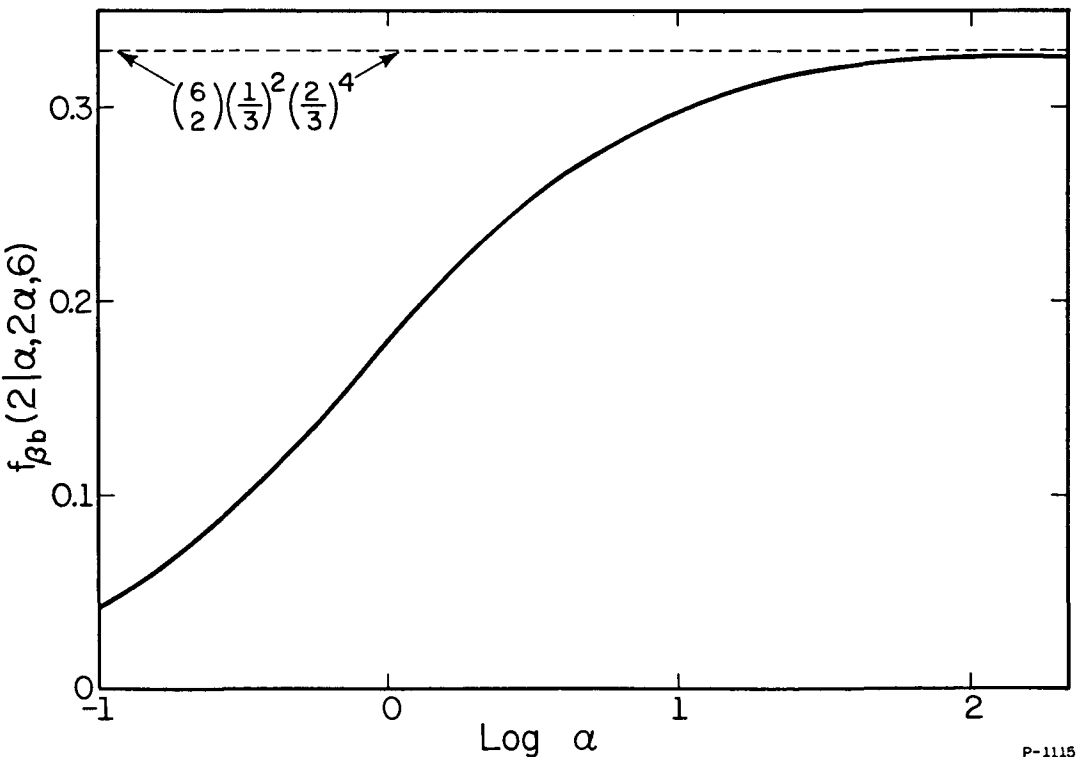
contributors, $\bar{m}$, are 2 and 1.512, respectively (Raiffa & Schlaifer, 1961, chap. 7). If three or more contributors are required for the provision of public goods ($m = 3$), $P_2 = 0.2273$ and $D(2, 4) = -2.727$, whereas if $m = 5$, $P_4 = 0.0519$ and $D(2, 4) = -4.481$. Because $D(2, 4) < 0$, player i should not contribute in either of these two cases.

Under the heterogeneity assumption, the probability of player i being critical is limited from below by 0 and from above by

$$\binom{n-1}{m-1} \left(\frac{\alpha}{\alpha+\beta}\right)^{m-1} \left(\frac{\beta}{\alpha+\beta}\right)^{n-m} . \tag{6}$$

Consequently, $-e < D(\alpha, \beta) < D(p = \alpha/(\alpha + \beta))$, where $D(p)$ and $D(\alpha, \beta)$ are given by equations (2) and (5), respectively. When both α and β approach 0, the beta distribution approaches a limiting two-point distribution with mass divided between $p = 0$ and $p = 1$. In this case, P_{m-1} ($1 < m < n - 1$) approaches 0 and $D(\alpha, \beta)$ approaches $-e$. Conversely, when α and β increase without bound, the beta distribution approaches a point distribution with all the mass on $p = \alpha/(\alpha + \beta)$. In this case, P_{m-1}

Figure 2. The Beta-Binomial Probability Mass Function Plotted as a Function of Log α



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approaches the expression in equation (6) and $D(\alpha, \beta)$ approaches $D(\alpha/(\alpha + \beta))$. Figure 2 depicts $f_{\beta b}(2|\alpha, 2\alpha, 6)$ plotted against $\log \alpha$. As α increases, $f_{\beta b}(2|\alpha, 2\alpha, 6)$ is seen to approach P_{m-1} in equation (2) with $n = 7$, $m = 3$, and $p = \alpha/(\alpha + 2\alpha) = 1/3$.

There are situations in which the unorganized group faced with the problem of the provision of public goods is divided into two or more subgroups, which differ or are perceived to differ from one another in their willingness or ability to contribute. Education, wealth, political attitude, age, or sex may either individually or collectively define such groups. For example, people with a college education may be more willing to contribute money to public radio than people without a college education; people who reside under or strongly believe in a centralized government may be less willing to donate blood or contribute to charity than people residing under or strongly believing in a decentralized government (Taylor, 1976); and subjects in an MCS experiment may share the belief that the females in the group are more likely to contribute their endowments than the males. In such cases, neither the homogeneity assumption nor the heterogeneity assumption would describe particularly well the expectations each member i has regarding the decisions of the remaining members of his or her group. Rather, the set N may be partitioned into subsets whose members are homogeneous among themselves, but behave independently of members of other sets (Straffin, 1978).

Formally, a *partial homogeneity structure* of N is a partition $P = (S_1, \dots, S_g)$ of N into g distinct subsets. A reasonable assumption is the following:

P-partial Homogeneity Assumption. Numbers q_h ($h = 1, \dots, g$) are selected independently from $[0, 1]$ and $p_j = q_h$ whenever $j \in S_h$. Given this assumption, the difference in expected value between contributing and not contributing may be computed as in the previous two cases.

Example. To illustrate the *P-partial-homogeneity* assumption, suppose that $n = 7$ and $m = 5$ as before, and that the group N is divided into two ($g = 2$) subsets, with $|S_1| = 3$ and $|S_2| = 4$. I write $D_h(P)$ for D in equation (1) to express the dependency of the difference $EV(C) - EV(\bar{C})$ under the partial homogeneity assumption on both the vector P and the group (subset) h .

Assume first that $i \in S_1$. Then of the remaining six members, two are in group S_1 and four are in group S_2 . To have a total of four contributors, there must be either zero

contributors from Group S_1 and four from S_2 , one contributor from S_1 and three from S_2 , or two contributors from S_1 and two from S_2 . Therefore,

$$\begin{aligned} D_1(P) &= rP_{m-1} - e \\ &= r\left[\binom{2}{0}q_1^0(1-q_1)^2 \cdot \binom{4}{4}q_2^4(1-q_2)^0\right. \\ &\quad + \binom{2}{1}q_1^1(1-q_1)^1 \cdot \binom{4}{3}q_2^3(1-q_2)^1 \\ &\quad + \left.\binom{2}{2}q_1^2(1-q_1)^0 \cdot \binom{4}{2}q_2^2(1-q_2)^2\right] - e \\ &= r[(1-q_1)^2q_2^4 + 8(q_1-q_1^2)(q_2^3-q_2^4) \\ &\quad + 6q_1^2q_2^2(1-q_2)^2] - e. \end{aligned} \quad (7)$$

Assuming next that $i \in S_2$, I obtain in a similar manner

$$\begin{aligned} D_2(P) &= r[3q_1q_2^3(1-q_1)^2 + 9(q_1^2-q_1^3)(q_2^2-q_2^3) \\ &\quad + 3q_1^3(1-q_2)^2] - e. \end{aligned} \quad (8)$$

With $e = 5$, $r = 10$, and $n = 7$ as before, equations (7) and (8) yield $D_1(P) = -2.037$ and $D_2(P) = -2.613$.

If $q_1 = q_2 = p$, then equations (7) and (8) reduce to (3) with $n = 7$ and $m = 5$.

Contributions in the van de Kragt et al. Experiment

Recall that in both conditions of the van de Kragt et al. experiment ($m = 3$ and $m = 5$) the public good was provided to 65% of the 7-member groups. Moreover, in most of the successful groups inefficiency occurred because of overprovision. Yet, whether the subjects' expectations in the experiment of van de Kragt et al. are most appropriately captured by the homogeneity assumption, the examples above show that the subjects should have never contributed. Following Dawes's (1980) discussion of cooperative behavior in social dilemma situations, the explanation that I favor for contribution in the MCS task is based on expected utility rather than expected value maximization. Individuals in decision-making tasks with externalities, including the subjects in the van de Kragt et al. experiment, have utilities that determine their behavior. These utilities are associated with aspects of behavior, such as altruism, social norms, and moral conventions, other than the external payoffs. In the MCS task (Table 1), the terminal payoff assumes one of four values: 0, e , r , or $r + e$. Because $r + e > r > e > 0$ and the utility

function is unique up to a linear transformation, I can set $u(0) = 0$ and $u(e + r) = 1$ with no loss of generality. The assumption that $u(r)/u(e) > r/e$ seems reasonable; it is in agreement with what is known about the existence of altruism, the operations of the social norm of cooperativeness, and the dictates of conscience. Given this assumption about the convexity of the utility function in the MCS task, the decision of whether to contribute may be explained by an appropriate selection of $u(r)$ and $u(e)$. This explanation is tautological unless $u(r)$ and $u(e)$ can be assessed independently.

To illustrate this explanation, suppose that $u(x) = ax^2$, where a is selected so that $u(0) = 0$ and $u(e + r) = 1$. For the parameters $e = 5$ and $r = 10$ in the van de Kragt et al. experiment, I obtain $a = 1/225$, $u(0) = 0$, $u(e) = 1/9$, $u(r) = 4/9$, and $u(r + e) = 1$. Substituting $u(r)$ and $u(e)$ above for r and e in equation (3), I have for $n = 7$ and $m = 3$,

$$D(p) = 4/9 \binom{6}{2} p^2 (1-p)^4 - 1/9,$$

with $D(p^*) = D(1/3) = 0.035 > 0$.

Social Implications

What sort of mechanisms may be devised by society to resolve the problem of the provision of lumpy public goods for small groups as abstracted by the MCS paradigm? An examination of equations (3), (5), (7), and (8) suggests that contribution toward the provision of public goods may be engendered by several complementary approaches, which fall in the realms of economics, education, and psychology. I sketch below three approaches, the first two of which have already been described and discussed for the related but more ubiquitous class of social dilemmas (Dawes, 1980; Messick & Brewer, 1983).

Like social dilemmas, the problem of public goods provision is defined in terms of the social payoff structure. Equations (3), (5), (7), and (8) suggest that the simplest proposal for engendering contribution is to change that structure by economic means that increase the r/e ratio. A change in the payoff structure would be produced by increasing the social benefit, by decreasing the size of the contribution, or by both. However, there are always practical considerations which limit the size that the ratio r/e may reach, and there is always the problem of who will institute the change and how (Dawes, 1980).

If, as argued above, the ratio of utilities $u(r)/u(e)$ rather than the ratio of payoffs r/e

matters, contribution could be enhanced by appeals to conscience, by rendering social norms more prevalent, by stressing the importance of altruism for society, and by depreciating self-motivation and selfishness. All of these mechanisms for eliciting cooperative behavior have been the subject of much theoretical discussion in education and practical use in religion.

Desirable as it may be, increasing the $u(r)/u(e)$ ratio may not be sufficient for ensuring contribution. For example, if $p = 1$, the individual should never contribute no matter how large is the $u(r)/u(e)$ ratio. Equations (3), (5), (7), and (8) show that, in addition to increasing the $u(r)/u(e)$ ratio, each individual's expectations about the decisions of the other members of his or her group should also be influenced, controlled, or manipulated. The experimental literature concerning attitude change may suggest mechanisms for changing belief structures. The analysis of the homogeneity, heterogeneity, and partial homogeneity models conducted above shows that individuals are more likely to contribute when they perceive their group as homogeneous: $D(\alpha/(\alpha + \beta))$ is always greater than $D(\alpha, \beta)$. Similarly, if the equality $q_1 = \dots = q_g$ in the partial homogeneity model is not satisfied, there exists a probability number, p^* , such that $D(p^*) > D_h(P)$. Hence, the first step in the third proposal is to convince each player that, with regard to the probability of contributing toward the provision of public goods, all the other members of his or her group are identical. The second step concerns the manipulation of p . Convincing each individual of the homogeneity of his or her group may not ensure contribution. As shown in Figure 1, even if $D(p^*) > 0$, contribution is optimal for some values of p (e.g., p^*) and not optimal for others (e.g., $p = 0$, $p = 1$). An implication of equation (3), which is experimentally testable, is that contribution toward the provision of public goods is most likely to emerge if each player is led to believe that the probability that any of the other players will contribute is at or very close to $p^* = (m - 1)/(n - 1)$.

Although I know of no direct experimental test of the above implication, the experimental results reported by van de Kragt et al. seem to support it. Van de Kragt et al. reported that subjects in the no-discussion condition "were, to some extent, tailoring their expectations about the incidence of contributing to the level of contributing that was required for public good provision" (1983, p. 118). My analysis of the homogeneity model similarly suggests that the expectations about the number of con-

tributors by a player who actually contributed will equal $(n-1)p^* = m-1$.

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The War Trap Revisited: A Revised Expected Utility Model

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The expected utility framework developed in The War Trap is revised to correct several deficiencies. Risk-taking orientations are now treated as an integral part of the model by introducing concavity or convexity into the utility functions. The zero-sum properties of the theory are largely eliminated, and the tendency toward interpersonal comparisons of utility is removed. Several earlier results are replicated with the new model, and with annual capabilities data. New propositions are deduced that identify important limitations on conflict initiation, and relationships resulting from differences in perceptions are tested. Support is found for the contention that the revised version of the theory, of which the original model is a special case, is a powerful tool for integrating many extant hypotheses about conflict and for explaining a substantial portion of the tendency for some threats to escalate to violence or warfare and for others to be resolved peacefully.

An expected utility approach to the study of international politics offers both the opportunity to deduce propositions about international conflict, and, through the application of admittedly crude indicators, to evaluate the usefulness of those propositions as explanations of actual behavior (Altfeld & Bueno de Mesquita, 1979; Berkowitz, 1983; Bueno de Mesquita, 1981a, 1983, 1984a; Morrow, 1982; Newman, 1982; Petersen, 1983; Wittman, 1979). Many studies that have applied such a framework to international conflict have used the model proposed in *The War Trap*. Although that model has proven helpful in explaining both intuitively obvious cases of international disputes and seemingly counterintuitive ones, still it possesses several serious shortcomings. I intend, therefore, to correct some of those shortcomings by proposing a refined version of the expected utility formulation set out in *The War Trap*. In doing so, I do not wish to suggest that the revised model corrects all of the weak-

esses in the earlier approach. Quite the contrary. Much still remains to be done, and work is going forward on improving the models further.¹ However, it seems useful at this juncture to introduce the modifications developed to date and some empirical analyses associated with them, so that others may react to the modifications and perhaps add to them.

As was pointed out in *The War Trap*, the weakest theoretical component of the expected utility approach as I constructed it was the establishment of four *ad hoc* decision rules used to specify necessary conditions for the initiation of conflict by risk takers or risk avoiders. Although these rules are generally consistent with the flavor of the concepts they are intended to capture, they represent a serious departure from standard treatments of risk.

My main objective here is to reconstruct the model so that it reflects risk through the introduction of concavity or convexity into the utility functions. In doing so, it is imperative that the model give each actor the opportunity to have a differently shaped utility function, with the extremity of the function's curvature embodying the extremity of the decision maker's willingness (or reluctance) to take chances.

A second objective, closely associated with the first, is to build greater protection against interpersonal comparisons of utility into the model (Zagare, 1982). Propositions concerned with the

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¹Appendix 1 contains the decision tree for a revision currently under investigation. All the results deduced from the current version of the theory can be deduced from this newer version. However, the version depicted in Appendix 1 is as yet untested.

escalation of conflict are best evaluated when one can estimate separately the perceptions of the key leaders in i and j . The revised model allows us to do exactly that. We can now estimate how each actor's situation looks as seen through another actor's eyes. Through this development it is possible to calculate the effects of differing perceptions on conflict decision making. Furthermore, it is possible to investigate an actor's decisions not only in isolation, but also in the context of his thinking about what he can do, and what his opponent can do in response (Maoz, 1983). In this way, the revised theory allows us to examine interactive decisions as well as independently made choices.

The alterations alluded to above have the additional characteristic that they greatly reduce the empirical tendency of the earlier model to produce zero-sum results in which i expects to gain exactly what j anticipates losing (Maoz, 1983). Although the theory as originally specified is not inherently zero-sum, still about 70% of the conflicts I studied yielded expected utility values that sum to zero. With the revised model, fewer than 10% of the cases sum to zero.²

Conceptualizing Risk-Taking in the Utility Functions

The central modification of the theory that is explored in this study is the introduction of an endogenously derived, continuous measure of risk-taking propensities. This measure permits the development of a potentially unique curvature to each nation's utility function for each choice situation in which it finds itself.

To evaluate risk-taking propensities I assume that leaders declare policy positions that represent some compromise between what they really want and what they believe is pragmatic or feasible. In particular, I assume that what one perceives is feasible is never more extreme than what one ideally wants. Then, using the original formulation in *The War Trap*, it is possible to estimate the degree to which i 's current policies leave i vulnerable to defeat, where such vulnerability is taken

to be an indicator of the feasibility of the policies being pursued. In particular, I define each nation's security level as $\sum_{j \neq i} E(U_{ji})$.³ The

greater this sum, the more utility i believes its adversaries expect to derive from challenging i . As this sum gets smaller and turns negative, i increasingly is in a position to extract concessions from j , and j is increasingly seen to be incapable of challenging i . In other words, as this sum decreases, i 's relative security increases, so that i is assumed to have adopted safe policies somewhat at the expense of i 's more extreme ideal.

One can identify the hypothetical policy portfolio that would maximize i 's security level (i.e., $\Sigma E(U_{ji})_{\min}$), and the hypothetical portfolio that leaves i most vulnerable to defeat (i.e., $\Sigma E(U_{ji})_{\max}$). How proximate i 's actual policies are to these extremes of vulnerability, in turn, may be taken as a reflection of i 's willingness to take risks. In particular, I assume that i 's risk acceptance increases as i 's security score approaches its level of greatest vulnerability, and that i ' risk aversion increases as its security approaches the level possessed by its safest policy portfolio. This risk-taking propensity is defined as:

$$R_i = (2\Sigma E(U_{ji}) - \Sigma E(U_{ji})_{\max} - \Sigma E(U_{ji})_{\min}) / [\Sigma E(U_{ji})_{\max} - \Sigma E(U_{ji})_{\min}].$$

This term is then transformed to:

$$ri = [1 - (R_i/3)] / [1 + (R_i/3)] \quad (1)$$

so that ri ranges between 2 and .5.⁴ As ri gets larger, i 's aversion to risks increases.

³The computation of expected utilities used to define the risk component is exactly the computational procedure found in *The War Trap* in which utilities are, for the moment, treated as if they are strictly a function of the similarity in foreign policy commitments of various actors. The utility functions will then have curvature introduced as a function of the proximity of each nation's actual policies to their safest and most dangerous alternatives. I am indebted to David Newman for suggesting this conceptualization of security. He has demonstrated that nations select alliance partners in a manner consistent with the notion that they are attempting to maximize their security as defined here (Newman, 1982).

⁴In the calculation of r_i it is necessary to transform R_i so as to prevent division by zero. That is why the equation calls for dividing R_i by 3. The particular divisor that is chosen will affect the rate at which the curvature of the utility function changes, but will not affect the direction of curvature. This measurement procedure for

²Furthermore, all the cases that sum to zero in the revised construction involve situations where $u_{ij} = U_{ij} = 1$, so that the respective expected utilities equal zero, meaning that the nations in question were extremely close allies. For these nations the unmeasured anticipated change in policy almost surely would have indicated deteriorating relations and, therefore, increased expected utility from waging war. Thus, their expected utilities, if fully estimated, would not have equalled zero, but would have been positive. For the logic underlying this statement see *The War Trap*, pp. 75-78.

The utility for success and the utility for failure may now be defined. Both of these utilities are assumed to be a function of the similarity of policy preferences across actors and the level of willingness to take risks within each actor. With U_{ii}^i being equal to the value i attaches to his own policy portfolio,⁵ and with U_{ij}^i being equal to the value i attaches to j 's policies as a function of their similarity to the policies of i , I may define the utility for success and failure respectively as:

$$U_{si}^i = 2 - 4[2 - (U_{ii}^i - U_{ij}^i)/4]^{ri} \quad (2)$$

and

$$U_{fi}^i = 2 - 4[2 - (U_{ij}^i - U_{ii}^i)/4]^{ri}. \quad (3)$$

Similarly, we may define the utility actor i attaches to the policy changes by his adversary that i anticipates will occur in the absence of a challenge by i as:

$$U_{qi}^i = 2 - 4[2 - [(U_{ii}^i - U_{ij}^i)_{in} - (U_{ii}^i - U_{ij}^i)_{ol}]/4]^{ri}$$

or equivalently, given that no change in policy is assumed,

$$U_{qi}^i = 2 - 4[(2-0)/4]^{ri}. \quad (4)$$

Of course, the U_{si}^i , U_{fi}^i and U_{qi}^i terms (with appropriate superscripts) are defined analogously. These terms vary as a function of whose estimate of expected utility is being calculated (i.e., who is the superscripted actor) by varying the risk exponent, so that for expected utility equations with an i superscript, calculations are done as specified above. For equations with a j superscript, j 's risk-taking propensity is used to estimate what j perceives to be the value of success, failure, or no

challenge for i in accordance with the equations delineated below.

I assume that for any superscripted actor $U_{si}^i \geq U_{qi}^i \geq U_{fi}^i$ (and equivalently for j), so that winning is better than or equal to no effort to change an adversary's policies.⁷ And not changing an adversary's policies is at least as good as trying and failing. That the definitions specified above are consistent with the structural assumptions found in standard treatments of risk taking is clear from the hypothetical utility functions based on these definitions depicted in Figure 1.

For the multilateral component of the expected utility equations which are developed below as extensions from *The War Trap*, risk is introduced into the utility functions through the following transformation:⁸

$$(U_{ki}^j - U_{kj}^j)' = (U_{ki}^j - U_{kj}^j)e^{Ri(U_{ki} - U_{kj})}. \quad (5)$$

This functional form assumes that risk averters undervalue support from friends and overvalue opposition from foes, whereas risk accepters overvalue support from friends and undervalue opposition from foes. Figure 2 depicts the effects of the multilateral risk-taking function for risk accepters, risk averters, and risk-neutral decision makers.

The Revised Model

The revised formulation is:

$$\begin{aligned} E^i(U_{ij}) = & [P_i(U_{si}^i) + (1 - P_i)(U_{fi}^i)] \\ & + \sum_{k \neq i, j} (P_{ik} + P_{jk} - 1)(U_{ki}^i - U_{kj}^j)' \\ & - [Q_{qi}^i U_{qi}^i] + (1 - Q_{qi}^i Q_{bi}^i)(U_{bi}^i) \\ & + (1 - Q_{bi}^i)(U_{wi}^i)] \end{aligned} \quad (6)$$

estimating risk-taking propensities was developed in close collaboration with Michael Horn and James Morrow, whose invaluable assistance I acknowledge.

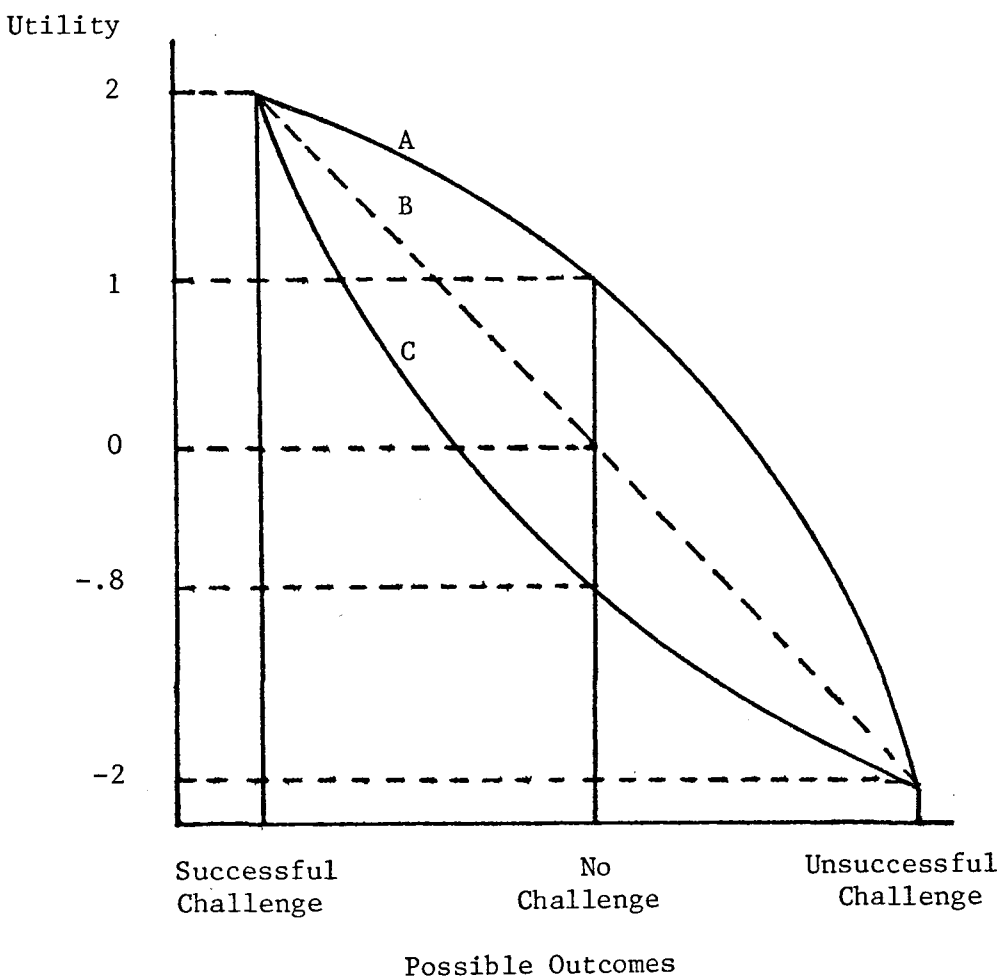
⁵Both U_{ii} and U_{jj} are assumed to equal 1.0, with U_{ij} and U_{ji} ranging between possible values of 1.0 and -1.0.

⁶As in *The War Trap*, all U_{ij} terms refer to the degree to which i and j share common foreign policy commitments. U_{ii} terms equal the value i attaches to its own policies, a value I define as being equal to 1.0. Once the risk-taking component is combined with these variables, the appropriate utility function is defined. The reason for the transformations by 2's and 4's is to preserve the original scale of numbers while avoiding the generation of imaginary numbers. Because ri can be less than 1.0, the absence of such transformations would mean that for negative values of, for instance, U_{fi} , no real root would exist. This problem is eliminated with the introduction of these transformations.

⁷We may also say that $U_{si}^i, U_{bi}^i \geq U_{fi}^i$ and $U_{si}^i, U_{bi}^i \geq U_{fi}^i$, although the relationship between U_{si}^i and U_{bi}^i is situation dependent, as is the relationship between U_{wi}^i and U_{fi}^i . This situational dependence adds considerable richness to the model, allowing decision makers to conclude, for instance, that a challenge is warranted even though defeat is expected. Such a situation arises if the expected value of j 's policy changes in the absence of a challenge is, from i 's perspective, inferior to the expected value for i of challenging j and losing. This, of course, can happen if i is able to impose sufficient costs on j to limit the amount by which j can alter its policies in the face of i 's opposition.

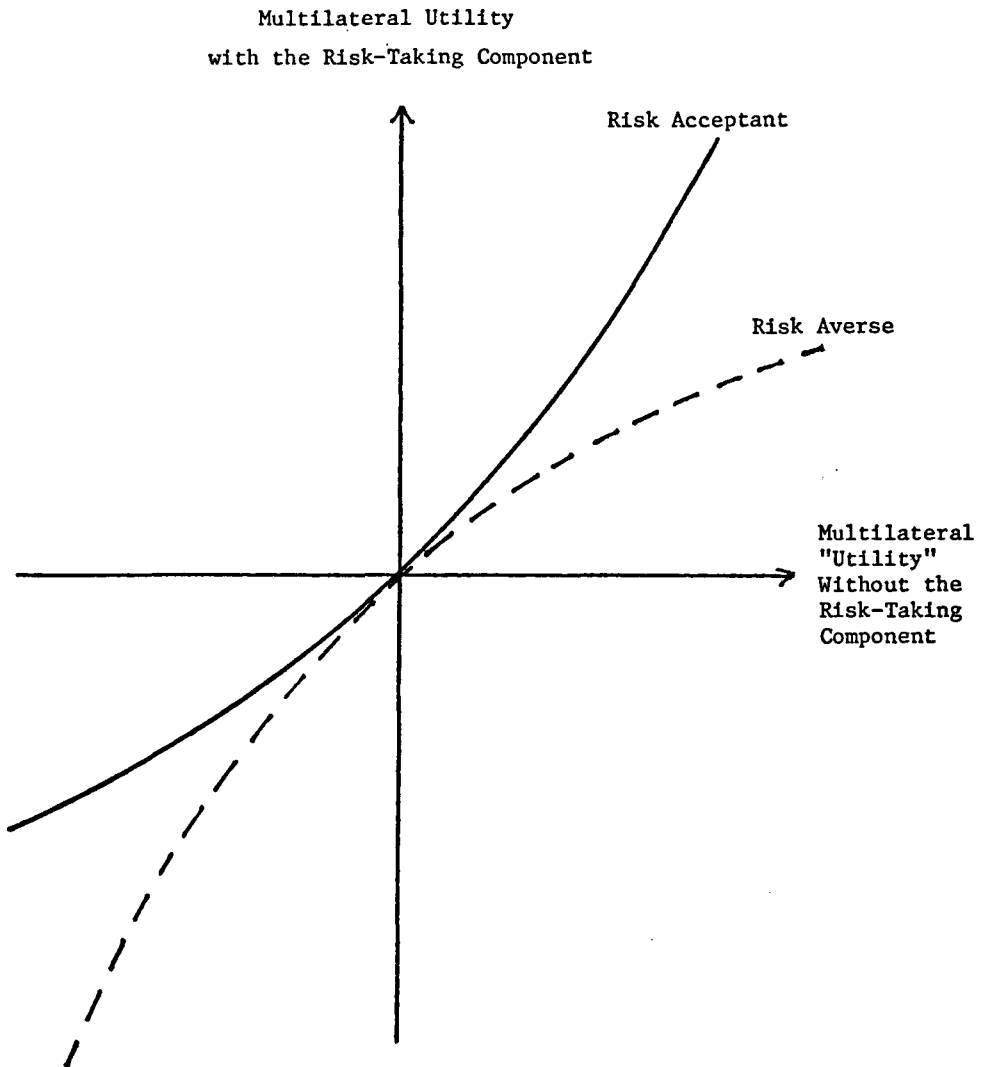
⁸In evaluating equation (5), the reader should keep in mind that all U_{ki} and U_{kj} are as defined in *The War Trap*, with the risk-taking component representing the innovation introduced here. That component is denoted by the use of a ' in the equation.

Figure 1. The Effects of Risk-Taking on the Curvature of the Utility Functions



Let $U_{aj} = U_{bj} = U_{cj} = -1$. Let $ra = 2, rb = 1, rc = .5$.
Let each actor's expected utility = the expected utility from challenging j minus the expected utility from not challenging j . Assume the probability of success from a challenge = .5, and assume the no challenge alternative yields the expectation of no change in policy by j for sure. Then:
 $E^A(U_{Aj}) = 0 - 1 = -1$. Therefore No Challenge is preferred to the lottery.
 $E^B(U_{Bj}) = 0 - 0 = 0$. Therefore (Challenge) Indifferent (No Challenge).
 $E^C(U_{Cj}) = 0 - (-.8) = .8$. (Challenge) Preferred to (No Challenge).

Figure 2. Multilateral Utility: The Effect of Risk Taking on the Function



$$\begin{aligned}
 E^j(U_{ij}) = & [P_j(U_{sj}^j) + (1 - P_j)(U_{fj}^j)] \\
 & + \sum_{k \neq i, j} (P_{ik} + P_{jk} - 1)(U_{ki}^j - U_{kj}^j) \\
 & - [Q_{qi}^j U_{qi}^j + (1 - Q_{qi}^j)(Q_{bi}^j(U_{bi}^j) \\
 & + (1 - Q_{bi}^j)(U_{wi}^j))]
 \end{aligned} \quad (7)$$

$$\begin{aligned}
 E^i(U_{ji}) = & [P_j(U_{sj}^i) + (1 - P_j)(U_{fj}^i)] \\
 & + \sum_{k \neq i, j} (P_{ik} + P_{jk} - 1)(U_{kj}^i - U_{ki}^i) \\
 & - [Q_{aj}^i U_{aj}^i + (1 - Q_{aj}^i)(Q_{bj}^i(U_{bj}^i) \\
 & + (1 - Q_{bj}^i)(U_{wj}^i))]
 \end{aligned} \quad (8)$$

$$\begin{aligned}
 E^j(U_{ji}) = & [P_j(U_{sj}^j) + (1 - P_j)(U_{fj}^j)] \\
 & + \sum_{k \neq i, j} (P_{ik} + P_{jk} - 1)(U_{kj}^j - U_{ki}^j) \\
 & - [Q_{aj}^j U_{aj}^j + (1 - Q_{aj}^j)(Q_{bj}^j(U_{bj}^j) \\
 & + (1 - Q_{bj}^j)(U_{wj}^j))]
 \end{aligned} \quad (9)$$

where

$E^i(U_{ij})$ = i 's perception of the difference in i 's expected utility from challenging j 's policies and from leaving j unchallenged. That is, this

term represents i 's expectation of its net benefit (or loss) from challenging j .

$E^i(U_{ji})$ = i 's perception of the difference in j 's expected utility from challenging i 's policies and from leaving i unchallenged.

$E^j(U_{ji})$ and $E^j(U_{ji})$ have analogous interpretations, but from j 's perspective.

P_i = i 's probability of succeeding in a bilateral contest with j . P_j is j 's probability of defeating i in a bilateral contest.

$(P_{ik} + P_{jk} - 1)$ = the marginal effect of third party k on the probability of i or j succeeding. This, and the associated utility terms, represent the algebraically reduced form of two lotteries, one in which k is assumed to join i , and one in which k is assumed to join j . Since on balance these are mutually exclusive alternatives (as are, for instance, i 's decision to select the strategies of challenging or not challenging j), the net impact of these two lotteries represents k 's marginal effect.⁹

U_{ki}^i = the value i believes is gained from support from k .

U_{kj}^i = the value i believes j gains from k 's support.

$U_{ki}^i - U_{kj}^i$ = the net value i believes will be contributed by k to the contest between i and j .

Analogous terms with j as the superscripted actor refer to j 's perception of k 's value to i and j respectively.

The U_s terms refer to the utility of success for the subscripted actor as perceived by the superscripted actor in the event the subscripted actor challenges the relevant adversary in a bilateral dispute. Thus, U_{si}^j is j 's perception of i 's utility for succeeding in forcing j to change its policies to be in accord with i 's wishes. The U_f terms are analogous to the U_s terms, except that U_f refers to the utility the superscripted actor believes the subscripted actor attaches to being defeated after its initiation of a bilateral challenge. These utilities are a function of the similarity in policies manifested by i and j , and of the risk-taking propensity of the superscripted actor.

The Q_q terms are the probability of the subscripted actor maintaining its current policies in the absence of a challenge by the other actor, with the estimate of that probability being made by the superscripted actor. For simplicity's sake, I will

assume throughout the rest of this study that the Q_q terms equal 1.0, so that the no-challenge lottery reduces to the assumption that in the absence of a challenge one's adversary is anticipated to maintain its existing policies.

The U_b and U_w terms refer to the utility the superscripted actor perceives that the subscripted actor attaches to some anticipated improvement or worsening of existing policy in the absence of a demand for policy change, whereas the U_q terms refer to the utility the superscripted actor perceives the subscripted actor attaches to no change in policy by its potential adversary. Despite the restrictive assumption that the relevant decision maker anticipates no change in its opponent's policies, the new risk-taking procedure that is introduced into the calculation of utilities preserves the existence of a distinct gambling threshold for each nation in each situation in which it finds itself.¹⁰ The expected utility associated with the no-challenge option represents the relevant decision maker's gambling threshold.

Thus, the expected utility from the challenge option must exceed the expected utility from the no-challenge option in order for a rational decision maker to choose to initiate an international dispute.

Equations (6) through (9) are equivalent to the original models in *The War Trap*, except that a) the utility functions are altered to allow concavity or convexity; b) the no-challenge side of the equation is modified so that a situation-specific gambling threshold is now theoretically defined for each nation; and c) analyses of the effects of perceptual differences, as reflected in the risk-taking component of the utility function, are now possible.

Derivations

For risk-neutral decision makers, equations (6) through (9) permit the derivation of all of the propositions found in *The War Trap*. In the absence of that constraint, however, these new equations can yield additional deductions. I turn now to the development of such propositions.

In *The War Trap* I showed that in order for i to be the initiator of a dispute in conflicts expected to remain strictly bilateral, it was necessary that i be as strong or stronger than j , so that $P_i \geq (1 - P_j)$. For strictly risk-neutral or risk-averse

⁹That $(P_{ik} + P_{jk} - 1)$ is the marginal contribution of k to the probability of success by i or j is easily shown. $P_i + (1 - P_i) = 1$. Let P_j (the probability that j succeeds in the bilateral contest) = $(1 - P_i)$. $P_i \leq P_{ik}$ since P_{ik} = the probability i succeeds given support from k . Similarly $P_j \leq P_{jk}$. Thus, since $P_i + P_j = 1$, $[P_{ik} + P_{jk} - (P_i + P_j)]$ must be the contribution of k .

¹⁰For a discussion of when this assumption is problematic, the reader should see *The War Trap*. The reader might also note that a measurement procedure for the no-challenge lottery has been developed and is now being tested. Results of those analyses will be reported in subsequent research.

decision makers that statement remains true whether one applies the original formulation or the revision proposed here. However, for risk-acceptant decision makers, it is not the case that initiation is dependent on $P_i \geq (1 - P_j)$. In general, assuming no third-party intervention, one may say that initiation requires that: $[P_i(U_{si}^i) + (1 - P_j)(U_{ji}^i)] \geq [U_{qi}^i]$ which, solving in terms of P_i , means

$$P_i \geq [(U_{qi}^i - U_{ji}^i)/(U_{si}^i - U_{ji}^i)] \quad (10)$$

Equation (10) is a necessary, although not sufficient, condition for the initiation by i of a conflict expected to remain strictly bilateral. Of course, equivalent relationships hold for equations (7) through (9). From this derivation one can see that as the value that i attaches to the status quo diminishes, approaching in the limit the value that i attaches to failure, i 's expected utility from conflict exceeds the expected utility from not challenging j even as $P_i \rightarrow 0$. Or, put differently, as the potential benefits from not challenging j (i.e., the numerator in equation (10)) increase, approaching in the limit, the potential stakes from challenging j , in order for i to initiate a dispute, i 's expectation of victory must increase. This is an important result for two reasons.

First, from equation (10) we see that the general law governing the initiation of a conflict expected to remain strictly bilateral does not require, as previously thought, that $P_i \geq .5$. If i is sufficiently willing to take risks, P_i may be considerably smaller than .5. Second, i 's willingness to take the risk of initiating a conflict when the probability of success is less than .5 means, of course, that under these circumstances i is likely to start a conflict that it will probably lose. This reminds us that the assumption of rationality does not require special insight by decision makers. Nor is that assumption's value properly called into doubt by the observation that some important war initiators suffered severe defeats. Quite the contrary. We can see how the rational actor perspective enhances our ability to understand "bad" decisions and perhaps even to anticipate the likelihood that such decisions will be reached. If, for instance, I substitute a risk-neutral perspective for the risk-acceptant perspective that i actually possesses, I can see precisely how much "excess" utility i derives as a result of looking at the world through the distorting lenses of a risk taker. Then I can estimate whether the "objective" situation would lead to the same expectations as those i possesses as a consequence of i 's willingness to gamble. In some circumstances, I may even be able to reduce the likelihood of conflict by identifying in ad-

vance the magnitude of distortion introduced by risk-taking propensities.¹¹

The solution to equation (10) and other derivations that can be constructed from the expected utility equations demonstrate the limits to bilateral-war initiation. These results show for the first time in a formal, explicit, structured way, just how large a gain or loss, just how large a chance of success or failure, a nation (or its leaders) can tolerate before being dissuaded from waging war. We can see clearly, for instance, that the realist paradigm's focus on power to the nearly total exclusion of all else (Morgenthau, 1973; Vasquez, 1983) is excessive.

Although power imposes real limits on international relations, so too do idealist considerations of policy or ideology, as embedded in the utility terms. We see also that such an individual personality attribute as the willingness to take chances can be incorporated in a systematic analysis of decisions for war or peace. Later I will show that differences in that attribute may be used to derive propositions about the likelihood of conflict escalation. For now, the focus on bilateral conflict should help to remind us that the expected-utility approach, with its focus on the utilities attached to alternative policy positions as well as with its focus on power (through the probability terms) and perceptions (through the risk-taking terms), incorporates much of what is central to many of the competing perspectives on international relations and does so in a theoretically structured, integrative, internally consistent manner.

Turning to the multilateral component of the calculus described in equations (6)-(9), I can extract additional insights that help to explain the conflict process while also demonstrating further the potential of the expected utility framework for integrating many extant theories of war. For instance, such power transition theorists as Organski and Kugler (1980) have argued that alliances are irrelevant when wars are waged between the two most powerful nations. Their reasoning is quite simple. They contend that the two strongest nations are so powerful by comparison with anyone else that allies are reduced to insignificance. They say, "Most of the time alliances are simply not a realistic method of preventing threatening changes in the distribution of world power, given the skewness of relations between the great and the lesser nations, and also among the half-dozen

¹¹In this regard, the model has already been used with considerable success as a tool for forecasting policy decisions in both a research context (Beck & Bueno de Mesquita, 1984; Bueno de Mesquita, 1984) and by government and private-sector organizations.

great powers themselves" (Organski & Kugler, 1980, p. 25). Stated in terms of the multilateral portion of equation (6) (or equivalently, equations (7), (8), or (9)), I can say: Let $P_j = (1 - P_i)$, so that $P_i + P_j = 1$, $P_{ik} \geq P_i$ and $P_{jk} \geq P_j$, so that $P_{ik} + P_{jk} \geq P_i + P_j$.

According to Organski and Kugler, one must assume that $(P_{ik} + P_{jk}) - (P_i + P_j)$, where $(P_i + P_j) = 1$, "given the skewness of relations between the great and the lesser nations." Then $(P_{ik} + P_{jk} - 1) \rightarrow 0$. Therefore, $(P_{ik} + P_{jk} - 1) (U_{ki}^{j_k} - U_{kj}^{j_k}) \rightarrow 0$.

Thus, a third party k has no impact on the expected utility of i or j when i and j are dominant powers engaged in conflict. The evidence offered by Organski and Kugler in support of their hypothesis that allies make no difference in power-transition wars is equally well suited as evidence for the expected-utility theory under the power conditions they specify.

Balance-of-power theorists suggest, however, in seeming contradiction to the power-transition argument, that it is exactly when two rivals are about equal in power that allies are critical in tipping the balance in favor of one side or the other, providing the margin of victory (Claude, 1962; Kaplan, 1957; Morgenthau, 1973). Yet the expected-utility theory demonstrates that this apparent contradiction is no contradiction at all. If, unlike Organski and Kugler, I assume that k does have some power, then it must be that $(P_{ik} + P_{jk})$ meaningfully exceeds $(P_i + P_j)$. In that case, barring k 's being indifferent between the policies of i and j , it follows that:

$$(P_{ik} + P_{jk} - 1) (U_{ki}^{j_k} - U_{kj}^{j_k}) \neq 0$$

so that, as suggested by balance-of-power theorists, alliances do make a difference. Thus, the expected-utility theory allows one to reconcile the seemingly contradictory alliance hypotheses of at least two central theories of conflict.

The logic of the multilateral component of the expected-utility formulation also permits us to see why major powers are more likely to become involved in conflict as third parties than are weaker nations:

Let k be a major power, and let i and j be weak nations. Then, in the limit, $(P_{ik} + P_{jk}) \rightarrow 2$, so that $(P_{ik} + P_{jk} - 1) \rightarrow 1$. So long as the difference between U_{ki} and U_{kj} is not zero, one can see that the multilateral component of the calculus is larger when k is assumed to be a major power than when (as in the power transition example), k is assumed to be relatively weak. Therefore, a "powerful" third party is presumed to be able to have an impact on virtually all conflicts, whereas a weak one can have an impact only under a special set of circumstances (in which, for in-

stance, i and j are about as strong as k). Major powers, then, are more likely to play a role in conflicts than are lesser nations (Altfeld & Bueno de Mesquita, 1979).

The revised model demonstrates that war (or lesser conflicts) can be initiated even when losses are expected, so long as those losses are not as large as the liability associated with continuing to live with the existing and anticipated status quo. The gamble of warfare becomes tolerable when the value of the status quo is sufficiently unattractive to offset the losses from war. In that sense, the total expected utility (as defined in equations (6)-(9)) must be positive, indicating either that an actual gain is anticipated or a smaller loss is expected from initiating conflict than from abiding the current situation.

Additional Hypotheses

The formulation stipulated above carries several important behavioral implications. First, since I am still modeling only necessary, and not necessary and sufficient, conditions, I can reiterate that decision maker i can, but need not, choose conflict as a strategy for acquiring policy gains (or reducing anticipated losses) only if equation (6) is not less than zero. This necessary condition is consistent with both the view that the decision maker in question is an expected utility maximizer or an expected utility satisficer. Only the specification of the calculus associated with *all* alternative strategies can allow us to distinguish between these two decision rules. Still, one can say that (H_1); equation (6) must be greater than or equal to zero in order for i to initiate a conflict with j .

Thus, by comparing equations (6) and (8), I can calculate i 's perception of the stream of events likely to follow i 's decision to initiate a dispute. Similarly, I can estimate j 's expectations regarding the unfolding of a dispute by comparing equations (7) and (9). To do so, I need merely to posit that i and j each assumes that the other is a rational, expected-utility maximizer or satisficer.

In order to specify the events that are likely to lead to the escalation of a dispute, I must first identify the conditions that define a dispute. A dispute or conflict occurs when i makes a demand of j which is accompanied by the threat to inflict punishment on j if the demand is not fulfilled. The demand may have many characteristics, of course: i may ask j to stop doing something that j was doing, for instance, or i may ask j to begin doing something that j previously did not do. The critical element is that the demand must be accompanied by a threat.

How can j respond to such a demand? Perhaps j can capitulate to the demand, or j can attempt to

negotiate with i in the hope of modifying i 's demand, or j can resist i 's demand, with force if necessary. Sometimes, when j attempts to negotiate and is unsuccessful, j will conclude that resistance (and fighting) is warranted. Indeed, one should expect that conflicts that escalate to include violence almost always pass through the negotiation stage, no matter how briefly. But when j capitulates to i , we should not expect further escalation except under situations where i 's perceptions are changed by the ease with which j gives in. Such situations, which may be characterized as describing the circumstance known as appeasement, should be relatively rare. By and large, then, one may perceive that j is expected to give in to i 's demands without resorting to negotiation or resistance if one believes that j perceives capitulation to those demands to be cheaper than resisting them.

One can represent all disputes as falling into one of four circumstances as perceived by one participant, so that 16 combinations of circumstances embody the complete perceptual mix possible among initiators and their foes. Figure 3 displays the four circumstances and provides the algebraic relationship each represents.

Escalation under Shared Perceptions

I will begin the discussion of Figure 3 by focusing first on the circumstances in which conflicts fall into the same portion of the Cartesian coordinates whether the dispute is viewed from i 's perspective or j 's. Suppose both sides perceive that the dispute falls in the first quadrant. Addressing the case from i 's point of view (just for ease of presentation), I can say that i believes i can extract a net benefit from challenging j . At the same time i believes that j expects to extract a net benefit from i . From i 's point of view this means that i does not expect j simply to give in to i 's demands, nor does i anticipate that a negotiated compromise is likely to be worked out. After all, how can adversaries compromise when both expect to be the recipient of net benefits? Because j shares the same perspective when j believes both sides expect to win, I can infer that war is highly likely under this "Fight" circumstance. I state this as the following hypothesis:

H₂: If i and j each believes their dispute falls in quadrant 1 of Figure 3, then the probability that their serious dispute will escalate to war will approach 1.0.

If the ensuing conflict is perceived by both parties to fall in section 2, in which one side is expected to win and the other to lose, but the loser is anticipated to lose less than the winner is demand-

ing, then neither party should expect simple capitulation to the demands being made. Instead, the side that is expected to lose should try to negotiate a compromise settlement. The negotiations will be over the difference in expectations reflected by the line segment AB in Figure 3. Some of the time these negotiations will succeed. At other times, however, one should expect that either i or j is unwilling to make a large enough concession, which leads to war. In general, that will happen when the anticipated costs of fighting and losing are smaller than the cost of the loss being demanded at the outset. I can state the general association as the next two hypotheses:

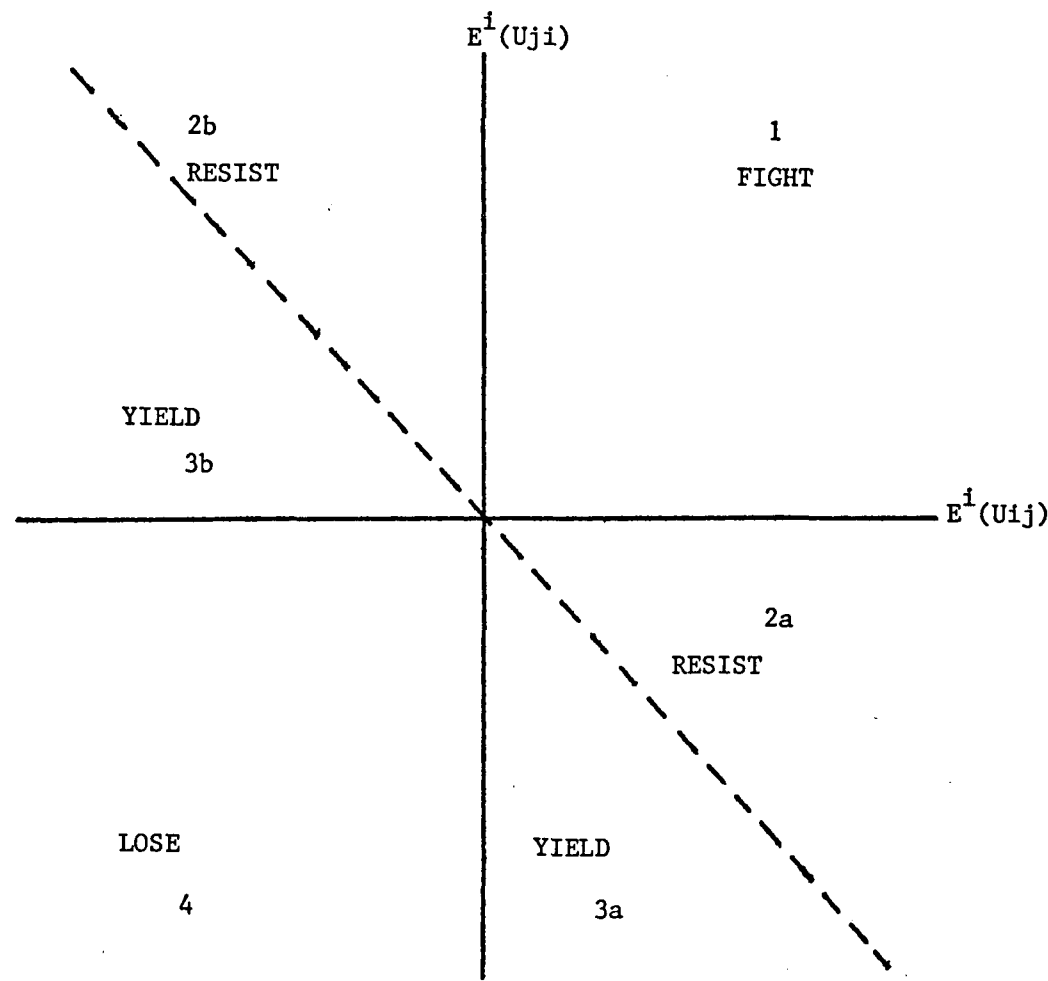
H₃: If both parties to a serious dispute agree on who the winner is anticipated to be, but disagree over how much the loser must give up to the winner, such that the loser believes less has to be sacrificed than the putative winner is demanding (i.e., the dispute falls in sector 2 of Figure 3), then some of the time a negotiated settlement will be reached, so that the conflict does not become as violent as a war, and other times the conflict will escalate to warfare. The probability of war under this Resist circumstance will be lower than under the Fight circumstance depicted in quadrant 1 of Figure 3.

When both initial disputants perceive that the expected utilities surrounding their conflict fall into sector 3, in which the potential victor is perceived to be capable of extracting a larger gain than that actor is perceived to be demanding, the expected loser is better off yielding to those demands than trying to negotiate or to fight for a better settlement. This is so because the loser anticipates that its adversary actually can gain more than is currently being sought. Any resistance to the opponent's modest demands could lead to an escalation in expectations. Certainly if the side making demands believes those demands are modest relative to what it is capable of extracting, it will have no incentive to accept even fewer gains in response to efforts at negotiations by the putative loser. Such conflicts should have a quite low probability of escalating to war, with the only likely exceptions being so-called appeasement circumstances, in which the loser yields so readily that the winner is encouraged to believe that additional benefits can be had cheaply enough to be worth pursuing.

H₄: If i and j agree on who is the expected winner of their dispute and if each side perceives the conflict falls in sector 3, called the "Yield" condition, then the probability of their dispute escalating to warfare (or violence) should be lower than the comparable probabilities under the "Resist" condition described in the previous hypothesis.

To continue with the circumstances in which i

Figure 3. Expected Utility:
The Effects of Perception on the Escalation of Conflicts



Fight: $E^i(U_{ij}) \geq 0$ and $E^i(U_{ji}) \geq 0$.
Resist_a: $E^i(U_{ij}) \geq 0$ and $E^i(U_{ji}) < 0$ and $|E^i(U_{ji})| \leq E^i(U_{ij})$.
Yield_a: $E^i(U_{ij}) \geq 0$ and $E^i(U_{ji}) < 0$ and $|E^i(U_{ji})| > E^i(U_{ij})$.
Lose: $E^i(U_{ij}) < 0$ and $E^i(U_{ji}) < 0$.

and j share a common perception about the nature of their dispute, I turn to situations in which the conflict falls in quadrant 4 of Figure 3. In this "Lose" circumstance, each side perceives that it cannot defeat the other side. Demands made under these circumstances are likely to be bluffs, posturing, or trial balloons. With luck one's adversary may give in to the bluffer's demands. Alternatively, one's adversary may respond with a bluff by assuming a tough, perhaps even threatening posture. Because neither side expects a net gain from concrete action, however, the conflict should not escalate beyond verbal threats. War should not take place.

H_5 : When both parties to a dispute perceive that they are facing a losing situation, the dispute should be resolved without warfare. The probability of war in such circumstances should approach zero, whereas the probability of a nonviolent resolution of the dispute should approach 1.

Escalation When Perceptions Differ

I now turn to disputes in which i 's perception of the relationship between i 's expectations and j 's expectations is different from j 's perception of that same relationship. Of course i 's and j 's perceptions may differ in two fundamental ways: i may anticipate that j 's incentives to negotiate or resist i 's demands are less than or greater than j 's perception of those same incentives. For instance, i may perceive that the i - j dispute falls into quadrant 1 (Fight) whereas j perceives that the dispute falls within the "Yield to i " sector (3a). The difference in such perceptions may have profound implications for the likelihood that a conflict will escalate to include violence or warfare.

When i makes a demand of j , i has some expectation about j 's response. If i believes j perceives its expected utility falls in quadrant 1, i expects a violent struggle. If i believes j 's expected utility falls into sector 3a which indicates that i is expected to defeat j , then i expects j to capitulate to its demands without negotiation or confrontation. But j , of course acts on j 's perception of the situation. Thus, if j perceives that the situation falls in sector 3 and that j is going to lose, then even though i might perceive j as falling in sector 2 (Resist), j will give in to i . Conversely, if, for instance, i thinks j falls into the Yield condition while j perceives that its dispute with i falls into the Resist circumstance, i will be surprised to discover that j , rather than giving in, will try to negotiate. Such a circumstance, in turn, is much more likely to lead to violence than if the circumstances were reversed, as in the first example. After all, i thinks it is making a modest demand of j —a demand for a settlement that is smaller than i thought j believed i could impose. If j tries to

negotiate under those circumstances (because j does not share i 's perception of the situation), i is likely to react with hostility toward j 's unreasonable reluctance to give i its modest request. One can state the circumstances covered by these and other situations that involve differences in perceptions as the following hypothesis:

H_6 : If i perceives that its conflict with j should not become violent because j is expected to give in to i , then if j does not give in, there is a higher probability of violence than if i perceives that j will resist or fight i when j perceives that it should give in to i 's demands; that is, when j 's perception of a dispute with i falls into a lower-numbered sector of Figure 3 than i 's perception of the same dispute, the probability of violence is higher than when j 's perception falls into a higher-numbered sector of Figure 3 than does i 's perception.

H_7 : As the dispute moves from the upper right quadrant of Figure 3 toward the lower left quadrant, the number of fatalities associated with the attendant conflicts should diminish steadily, approaching zero in quadrant 3.

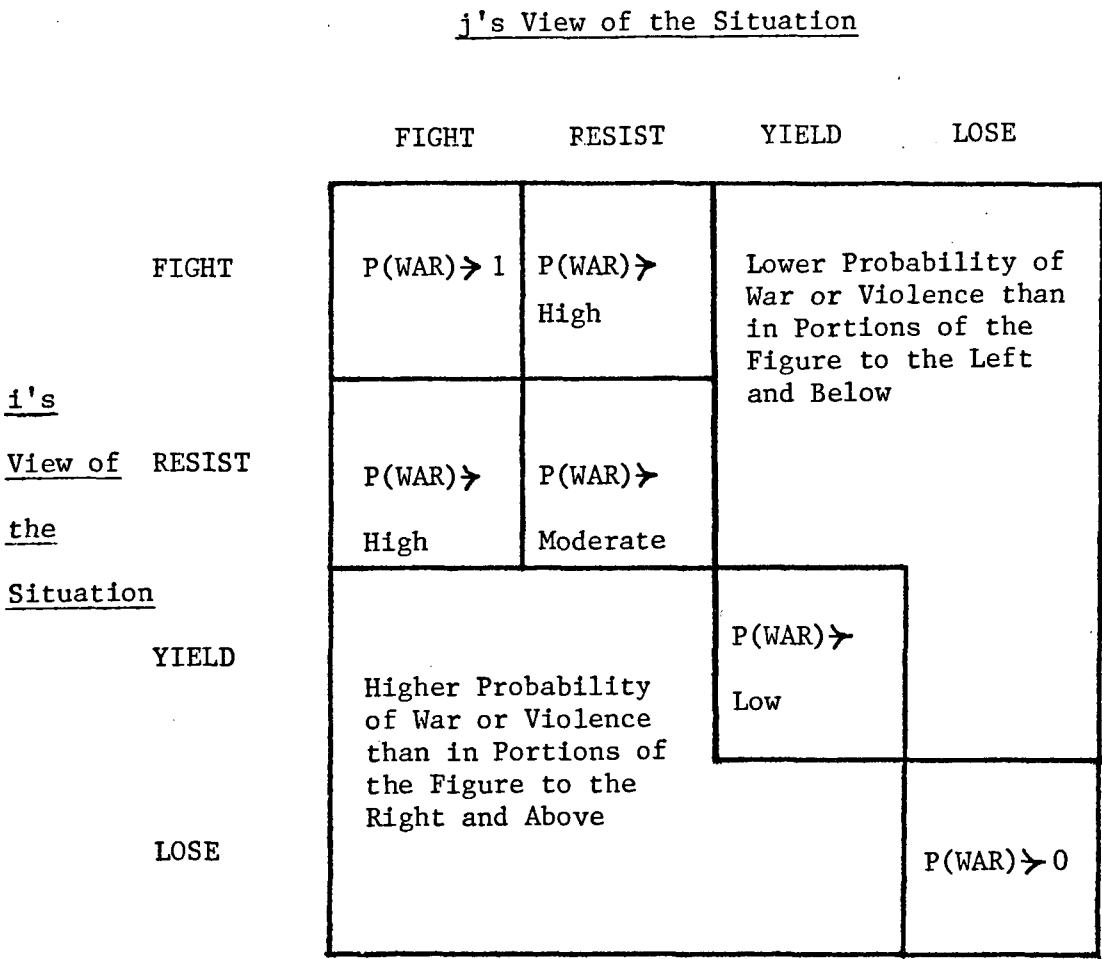
Hypotheses two through seven are summarized by the relations depicted in Figure 4. None of these "escalation" hypotheses seems particularly surprising or counterintuitive. Indeed, once the logic of expected utility decision making has been laid out, these hypotheses seem almost obvious. Yet when we turn to their empirical investigation we should not forget that they are not ad hoc hunches, but a direct consequence of a systematic, comprehensive theory that provides hypotheses about conflict initiation, escalation, and termination, as well as about deterrence, detente, and alliance formation. Furthermore, we should bear in mind that they are obvious because of the expected utility framework. Without that framework we could not state these relationships. Indeed, to the best of my knowledge, they appear nowhere else in the literature on crisis management or conflict resolution. Yet many of these hypotheses are fundamental statements about the significance of perceptual differences which play so important a role in much of the conflict literature.

Indeed, several of these represent genuinely testable hypotheses about the relationship between conflict and perceptions.

Research Design

To test the revised model, I focus on 133 European disputes that took place between 1816 and 1965. These disputes include the 51 threats, 48 interventions, and 34 wars, identified in Appendix 2. Equations (6) through (9) were solved for all the

Figure 4. Graphic Depiction of Hypotheses 2-7



European dyads from 1816-1965, the period for which I have complete data.¹² For these analyses, I used the Correlates of War Project's *annual* composite capabilities data from 1816 to 1965. For the handful of conflicts within my data set for which annual data were incomplete I used the closest data point to the conflict. In practice, this meant that the scores for one dyad in 1914 are

based on 1913 data, two 1918 dyads are based on 1919 data, and three 1940 dyads are based on 1939 data. All measurement procedures are the same as those described in *The War Trap*, with the exception of the assessment of risk-taking orientations.

The risk measure, which is described in greater detail in Morrow (1982), is predicated on the notion that national leaders select their security policies with an eye to what they desire and also with an eye to what they think they can safely get away with. Thus, the risk terms are calculated by manipulating the alliance portfolios used as the policy indicator through simulation to locate the best and worst portfolios for any given nation, where the best and worst are defined in terms of the sum of expected utilities of all others vis-à-vis the nation in question under the assumption that

¹²I focus only on European dyads and European disputes here because of the costs of solving the revised model. Since Europe experienced more disputes for which I have data than any other geopolitical region, I begin with that part of the world. In subsequent analyses, as funding permits, I will extend the investigation to all of the geopolitical environments examined in *The War Trap* and to alternative constructions as well.

utilities are strictly a function of similarities in alliance commitments.¹³ That is, the combination of hypothetical alliance commitments for nation i (holding all other existing alliance commitments constant) that leads to the smallest possible sum of expected utilities for each j versus i is found, indicating i 's most secure position. Similarly, the worst hypothetical combination of alliance commitments is identified. These sums define the terms used to measure R_i in equation (1). I then calculate i 's propensity to take risks as a function of where in the range of possible security levels i 's actual alliance commitments place i . If i is exactly at the midpoint of the range, $R_i = 0$. As i moves toward a more risk-acceptant posture (i.e., away from more defensible policies toward less defensible policies), R_i increases, and as i moves closer to its most secure alliance portfolio, R_i decreases. The final risk score, calculated according to equation (1), is constrained so that R_i ranges between $+2$ and $.5$. Some constraint is required, as noted earlier, to avoid division by zero. These risk scores are then introduced into the utility functions as described in equations (2) through (5).

Results

I begin my examination of the analysis by evaluating, albeit briefly, the results of the risk-taking calculations. It will be recalled that an important limitation of the initial measurement procedure for risk taking as described in *The War Trap* was that risk scores could not be estimated for major powers, compelling me to treat all such nations as if they were risk neutral. This was unfortunate, as the distribution of risk-taking propensities is a subject of considerable import for much of the research on war.

I have noted elsewhere that the theoretical relationship between systemic polarity and war (Bueno de Mesquita, 1978), or the theoretical association between power distributions and the likelihood of war (Bueno de Mesquita, 1981b), for instance, is dependent on what is assumed to be the distribution of risk taking among key national leaders. The contending polarity hypotheses arise because of different implicit assumptions about risk-taking propensities. Deutsch and Singer (1969), who contend that bipolarity tends to pro-

duce war whereas multipolarity tends to yield peace, seem inclined to believe decision makers are generally risk averse, whereas Waltz (1964), who supports the opposite hypothesis, appears to assume that such leaders are typically risk acceptant. I have argued that if risk taking is normally distributed, neither the hypothesis that bipolarity leads to war nor the counterhypothesis that multipolarity leads to war should be generally correct (1978).¹⁴ I have made similar arguments with respect to the contending hypotheses linking power distributions to war (1981b).

What, then, is the distribution of risk-taking propensities using the revised formulation? Risk scores were calculated for each European nation for each year of membership in the international system, for a total of 3,332 annual nation risk scores. The mean R_i value for these cases is .018, with a standard deviation of .503 and a range of values from -1.00 to $+1.00$. The median is .182. In other words, the average score is just about exactly at the risk neutral point. The measures of kurtosis and skewness for the distribution indicate only a moderate deviation from a normal distribution, with that deviation indicating slight skewness toward risk acceptance.¹⁵ The distribution for the major powers was essentially the same as for the lesser nations, with the range of obtained values being identical. Turning to the participants in the 133 pairwise serious disputes investigated here, I find that the overall mean risk score is .026, with the mean for initiators $-.033$, and for targets .085. *The means for all the conflicts tend toward risk neutrality, and the distribution is essentially normal.* Such a distribution casts serious doubts on many prominent hypotheses linking either polarity or power to war. But is there reason to believe that the risk scores estimated

¹⁴In fact, one need not assume a normal distribution of risk-taking propensities to cast doubt on the polarity or power hypotheses. It only need be the case that risk orientations are widely dispersed, rather than heavily skewed toward risk acceptance or risk aversion.

¹⁵Interestingly, the mean risk-scores for the two centuries are significantly different from each other. The nineteenth-century mean of .064, indicating a slight tendency toward risk-acceptant behavior, stands in contrast to the twentieth century mean of $-.039$. The t -statistic associated with these means is 5.982. The t -statistic for the intercentury difference in risk scores for major powers is also significant ($t = 3.856$). In this case, the analysis indicates that although major powers tended to be slightly risk averse in the nineteenth century, their leaders became more risk averse in the twentieth century. These changes carry important implications which will be investigated in a subsequent study. I wish to thank Michael Horn for his invaluable assistance in calculating these distributional properties.

¹³That is, temporarily applying the expected utility equations (without risk or uncertainty taken into account) as developed in *The War Trap*, I identify the worst case and best case alliance strategy for each nation each year, using the original, linear utility functions to define the range of possible expected gains or losses for each nation. These, then, are utilized to measure risk propensities and thereby to introduce curvature into the utility functions.

here are related to actual behavior? To answer this question, let us examine Figure 5. If the indicator of risk taking is meaningful, we should expect that those who were risk acceptant had a substantially higher probability of initiating unsuccessful violent conflicts—wars or interventions—than did those who were risk averse. As can be seen in Figure 5, 13 of 40 risk-acceptant initiators (or 33%) lost their violent conflicts, whereas only 2 of 40 risk-averse initiators (or 5%) suffered a similar fate. The mean risk score among defeated initiators of violent disputes is .286, whereas the mean among victorious initiators is -.139. The difference is significant at less than the .05 level. Apparently risk-acceptant initiators prove to have a much higher probability of defeat than do risk averse initiators, which suggests that they do, indeed, take greater risks.

The analysis, based on the model as presented in *The War Trap*, of the 133 cases included here shows that 107 of 133 initiators had positive expected utility, with only 31 opponents similarly possessing positive expected utility.¹⁶

¹⁶Because I have now shown that in conflicts expected to remain bilateral it is possible for P_i to be less than .5

Yule's Q , a measure of necessary, but not necessary and sufficient, conditions, equals .86 for this analysis. The original model, solved using annual rather than quinquennial capabilities data, shows that 106 initiators had positive expected utility, while 39 opponents were similarly endowed. Yule's Q for this analysis is .81, or essentially the same as with the quinquennial data. Making similar comparisons using i 's expected utility scores based on i 's perception of its own potential (equation (6)) and j 's expected utility scores based on j 's perceptions of its potential (equation (9)) demonstrates that 103 initiators had positive expected utility, 35 opponents were so endowed, and Yule's $Q = .81$. There is essentially no difference in goodness of fit when I compare the original model solved with quinquennial data to its solution based on the annual capabilities

and for i still to initiate a conflict under the rules of rationality, I no longer treat expected utilities of zero for the weaker side as being necessarily less than zero based on expectations regarding the utility associated with anticipated future policy positions. Instead, all zeros are now treated as positive numbers, in accordance with the expectations derived from the modified theory.

Figure 5. Relationship between Risk Taking, War Initiation, and Victory or Defeat

		i Won	i Lost
i Is:	Risk-Acceptant	27	13
	Risk-Averse	38	2

data, nor does the switch from the original theoretical form to the new form seem to have mattered. In the original formulation, 107 out of 133 initiators had positive expected utility; 103 out of 133 are similarly endowed in the latest formulation. The difference is not statistically significant. The expected number of initiators with positive expected utility from among the 133 initiators, given the distribution of this attribute in the nearly 80,000 annual European dyads is substantially less than that which is observed. In fact, the difference is so large that the probability that 103 out of 133 initiators had this attribute by chance is less than .001, with $z = 6.281$.

These results seem to be quite encouraging. They demonstrate that the results are rather robust. They also show that the new version of the theory, which is both less ad hoc in its treatment of risk taking and is more parsimonious (having eliminated the need for four discrete decision rules), works as well as the more complicated original version when a simple hypothesis (H_1) linking the sign of expected utility to conflict initiation is examined.

Although these results provide some interesting information, they really are not able to show the key differences between the revised model and the original formulation. To see these, I must turn to the hypotheses that focus on conflict escalation. Here I can explore perceptual differences between i and j more thoroughly. In the original model it was not possible to distinguish between i 's expectations and j 's without making interpersonal comparisons of utility. With the new formulation, no such interpersonal comparisons are required. Because equations (6) and (8) show i 's expectations and equations (7) and (9) similarly indicate j 's, I can evaluate hypotheses (2)-(7) on conflict escalation and perceptions in a manner not previously possible. Let me turn, then, to an examination of the extent to which this added dimension contributes to our understanding of conflict escalation. The results are summarized in Figure 6.

Hypothesis 2 stipulated that the probability of war approaches being equal to one given that both sides perceive the situation as "Fight." Of the 12 disputes that satisfy this condition from both i 's and j 's perspective, 11 (92%) became wars. It is difficult to assess whether or not .92 is statistically different from 1.00, although it seems pretty clear that there is not a substantive difference. Using the normal approximation to the Bernoulli distribution and assuming that the expected proportion of wars equalled .98 demonstrates that the observed value of .92 is not significantly different from the expected value. It appears, then, that the probability of war under this circumstance does approach 1.00.

Hypothesis 3 indicates that the probability of war under the Resist condition should be significantly lower than under the Fight circumstance. Of the 29 disputes for which both i and j shared the perception that the conflict fell within the Resist category, 10 (34%) became wars. This is significantly less than 92%, with the attendant z -score being 4.168.

Hypothesis 4 claims that the likelihood of war under the Yield condition is still lower, and indeed it is. When both sides perceive their conflict to require a Yield (as they did 33 times), only six conflicts (18%) escalated to warfare. The attendant z -score comparing Yields to Resists is 2.147, which indicates a significant difference.

Hypothesis 5 postulates that under the Lose circumstance conflicts do not become wars, so that the probability of war should approach zero. Of the six cases that satisfy the mutual Lose conditions of this hypothesis, five were verbal threats and one involved an intervention in which there was a loss of between 101 and 250 lives. These results support the expectations found in the fifth hypothesis.

Hypothesis 6 focuses attention on situations in which the antagonists in a serious dispute perceive the situation differently. In particular, this hypothesis draws attention to the fact that the decision to escalate lies primarily with the target of a hostile demand. This is not because the target perceives more accurately than the initiator, but because the target acts on his perceptions just as the initiator acts on his perceptions. Thus, if the target believes that capitulation is appropriate, it really does not matter that the initiator was prepared for strong resistance from its opponent. It is completely within the control of the target to yield. Likewise, if the initiator perceives that its demands should prompt a quick surrender, that will not deter the target from fighting if the target perceives fighting to be appropriate. What do we find?

The association is in the predicted direction, but there is not a significantly higher probability of war when j perceives the situation to warrant resistance than when i does. When only i perceived that war was possible (i.e., i perceived the situation to be in the Fight or Resist categories), 0 out of 10 disputes escalated to warfare. When only j thought war was possible (i.e., only j perceived the situation to be Fight or Resist, 5 of 40 events became wars. However, there is a much higher probability that the conflict will escalate beyond a mere verbal threat to include violence. Less than one-fourth of the conflicts in which i 's perception of the prospect of violence was greater than j 's perception actually became violent, whereas 25 of 40 disputes (62.5%) escalated to include violence when j 's perception of the prospect

Figure 6. The Distribution of Wars and Interventions

<i>i</i> 's View of the Situation	<i>j</i> 's View of the Situation			
	Fight	Resist	Yield	Lose
Fight	11/12=.92 ^a	2/3=.67		
	11/12=.92	2/3=.67		
Resist	No Cases of Conflict in this Cell	10/29=.34 22/29=.76	0/10=0 1/10=.10	
Yield	5/40=.13 25/40=.63		6/33=.18 20/33=.61	
Lose				0/6=0 1/6=.17

^aThe upper set of numbers in each cell represents the number of wars divided by the total number of conflicts falling within the cell category. The lower set of numbers represents the number of violent conflicts (wars and interventions) divided by the total number of conflicts (wars, interventions, and threats) falling within the cell category.

of violence was greater than *i*'s. This difference is significant, with the attendant z-score being 5.912.

Let us now turn to the seventh hypothesis, in which the level of fatalities across the conflict categories is discussed. It will be recalled that I hypothesized that Fight situations generally escalate to a more severe level of violence than do disputes in the Resist category. I can add considerable discrimination beyond the threefold categorization of War, Intervention, and Threat to describe disputes. Gochman and Maoz (1982) have coded disputes in terms of the number of fatalities, using a six-fold scheme. Cases in their category 1 had no fatalities, whereas those in cate-

gory 2 had 1-25 fatalities. Category 3 involves disputes in which 26-100 fatalities occurred, whereas category 4 includes 101 to 250 deaths. Category 5 disputes experienced between 251 and 500 fatalities, whereas disputes in the sixth category had more than 501 deaths (and included all Singer and Small wars). Fatality category 6 was both the median and modal category for conflicts satisfying the Fight condition. Although this is a prominent category for disputes involving the Resist condition, the modal category is 1, indicating that many Resist disputes were resolved by peaceful means, with no fatalities. The distribution of fatalities across these two conditions is significantly different and in the predicted direction,

with the strength of that difference so large that it would have occurred by chance fewer than one time in one hundred. Interestingly, each adjacent dispute category—from Fight to Resist, from Resist to Yield, and from Yield to Lose—involves an equivalently significant decline in the level of associated fatalities. Indeed, the strength of the decline is so marked that if I compare the two extreme situations—Fight and Lose—with respect to fatalities, I find Kendall's tau equals $-.64$, with the associated probability that this association has occurred by chance being less than 1 in 10,000.

All seven hypotheses have been strongly supported by the evidence. We have found that the revised model is a powerful tool for predicting conflict escalation, with respect to both movement from nonviolent to violent conflict and to the level of fatalities associated with the use of violence. As we move down the quadrants in Figure 3 we have found that the probability of violence drops, and when violence occurs, its magnitude also drops. The results demonstrate that differences in perceptions have a profound impact on conflict escalation. When the target believes it has a credible prospect of extracting some advantage from fighting, violence is several times more likely than when only the initiator believes the target could derive such an advantage. What is more, the patterns of escalation show that nations behave in a manner fully compatible with the expected utility perspective, taking risks when leaders believe those risks are warranted, and avoiding them otherwise. Indeed, I should close this discussion by noting that when i chooses to begin a war or lesser conflict, his estimate of i 's and j 's expected utility is a rather good forecaster of the eventual outcome of the dispute. The winner possessed positive expected utility 81% of the time (with that for wars 88%). Given the distribution of positive and negative expected utility scores among the roughly 80,000 annual European dyads investigated here, or among the initiators and opponents that participated in the conflicts under study, I would expect so large a proportion of winners to have positive expected utility by chance far fewer than one time in 10,000. Yule's Q for the association between i 's expected utility estimates and the subsequent conflict outcome is $.81$, with a proportionate reduction in error of over 50%. For violent conflicts (wars and interventions), Q is $.89$, and the proportionate reduction in error is nearly 60%.¹⁷ Thus it is

obvious that i 's estimates (as calculated here) of the solutions to equations (6) and (8) are strong predictors of the actual resolution of the ensuing conflicts (with j 's estimates from equations (7) and (9) being equally good), with nearly all of the deviations from i 's apparent expectations being, as noted earlier, the consequence of i 's willingness to take risks.

Conclusions

Although a great deal of progress remains to be made in the development and testing of a theoretically sound explanation of conflict decision making, the results still seem encouraging. The expected utility framework has been modified to correct several deficiencies contained in its earlier formulation, which has allowed the deduction of additional propositions, and the evaluation of significant aspects of the relationship between perceptual differences across decision makers and subsequent conflict patterns. The new results do not negate any of the findings reported in *The War Trap*, but they do build from them and enhance them. The new results point strongly in a direction that indicates that one can predict with considerable confidence the likelihood of a conflict or threat becoming a war. Because many of the theory's terms are manipulable, these results suggest new directions in conflict resolution and conflict management. The results continue to support the contention that this approach may have yielded significant, lawlike generalizations about the initiation, escalation, and termination of international conflict.

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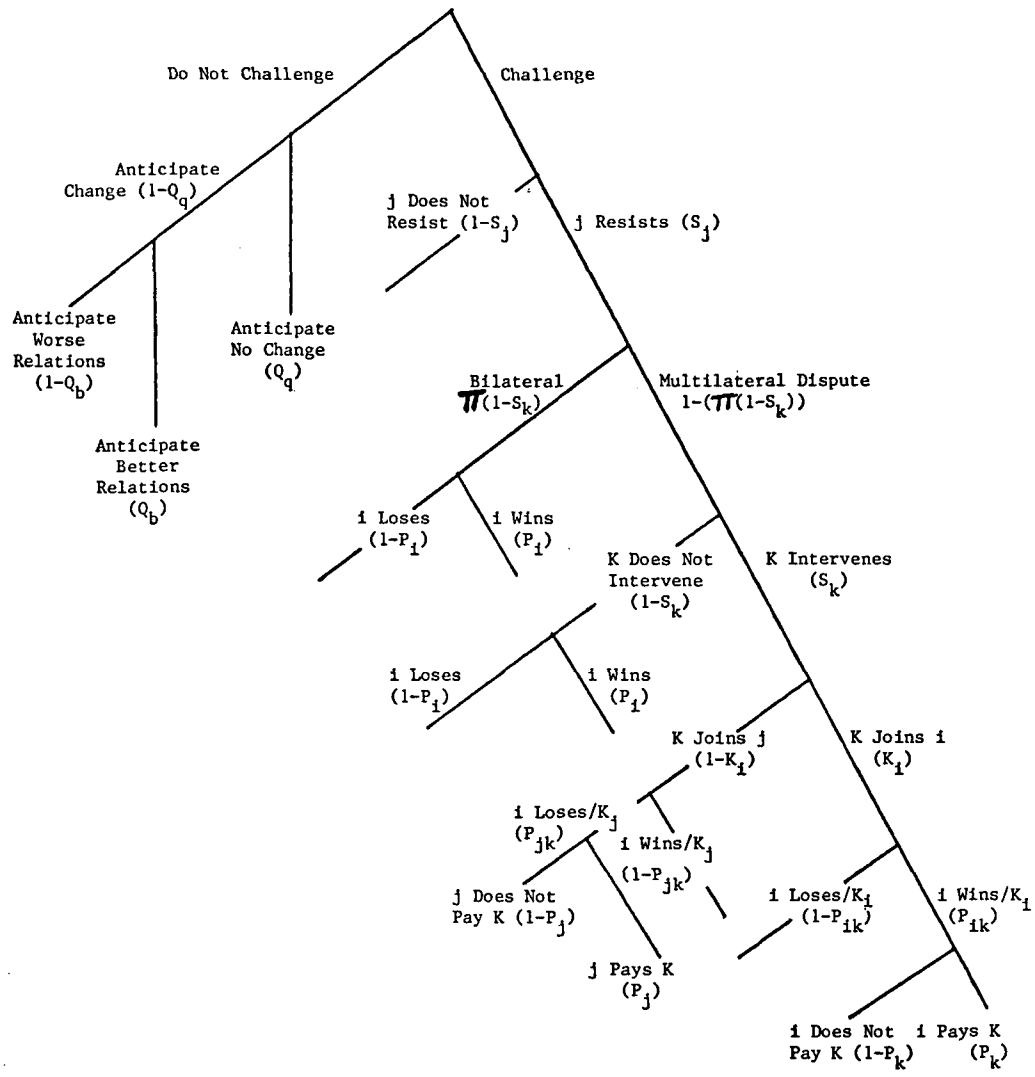
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¹⁷These values, of course, *underestimate* the goodness of fit between the data and the theory. It will be recalled from this discussion, as well as from *The War Trap*, that there are several circumstances under which the theory leads us to expect the initiator to lose. One such

circumstance discussed in this study involves risk acceptant, weak initiators of bilateral conflicts. Another, discussed in *The War Trap*, involves a situation of post-initiation deterrence (see also on this subject Huth & Russett, 1984). Still other such circumstances can be deduced from the expected utility framework.

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Appendix 1. The Decision Tree for a Further Revision



Appendix 2. Expected Utilities Estimated with the Revised Model

Year	Initiator	Opponent	$E^i(U_{ij})$	$E^i(U_{ji})$	$E^j(U_{ij})$	$E^j(U_{ji})$	R_i	R_j
Wars								
1823	France	Spain	.536	-.944	1.072	-1.223	-1.000	.463
1827	UK	Turkey	.240	-.413	.773	-.818	-.958	.398
1827	France	Turkey	.377	-.674	.713	-.919	-.956	.398
1827	Russia	Turkey	.203	-.455	.668	-.680	-.957	.398
1828	Russia	Turkey	.218	-.510	.738	-.797	-.959	.354
1848	Italy	Austria	-.624	1.469	-.981	1.683	.493	.112
1848	Germany	Denmark	.342	-1.454	.993	-.943	-.700	.510
1849	France	Papal	.857	-.664	.298	-.175	.269	.779
1853	Turkey	Russia	-.543	.858	-.627	.912	.371	.253
1859	Austria	Italy	1.350	0.000	1.512	-.320	.427	-.320

APPENDIX 2 (continued)

Year	Initiator	Opponent	$E^i(U_{ij})$	$E^i(U_{ji})$	$E^j(U_{ij})$	$E^j(U_{ji})$	R_i	R_j
1860	Italy	Papal	.424	-.206	.380	-.169	.430	.506
1860	Italy	Sicily	.274	-.057	.265	-.049	.430	.461
1864	Germany	Denmark	1.385	-1.352	1.272	-.588	-.186	.550
1866	Germany	Austria	0	0	0	0	.115	.260
1866	Germany	Baden	0	0	0	0	.115	-.919
1866	Germany	Bavaria	0	0	0	0	.115	-.786
1866	Germany	Saxony	0	0	0	0	.115	-.886
1866	Germany	HesElec	0	0	0	0	.115	-.951
1866	Germany	HesGran	0	0	0	0	.115	-.947
1866	Germany	Wrtlnbrg	0	0	0	0	.115	-.908
1866	Germany	Hanover	0	0	0	0	.115	-.864
1870	France	Germany	.298	.016	.281	.029	.394	.494
1877	Russia	Turkey	.692	-.968	.793	-.502	-.194	.374
1897	Greece	Turkey	-.297	.523	-.454	.648	.457	.250
1911	Italy	Turkey	.614	-2.066	1.262	-1.571	-.640	.192
1912	Yugoslavia	Turkey	-.490	.836	-.753	1.041	.473	.200
1913	Bulgaria	Yugoslavia	0	0	0	0	.520	.546
1913	Bulgaria	Greece	0	0	0	0	.520	.530
1914	Austria	Yugoslavia	1.499	-2.153	1.587	-1.095	-.391	.546
1919	Rumania	Hungary	.357	-.143	.345	-.129	.370	.404
1919	Greece	Turkey	-.004	.216	-.003	.215	.354	.361
1939	Germany	Poland	.269	-.517	-.017	-.468	-.205	-.524
1939	Russia	Finland	.838	-.857	.830	-.827	-.041	.016
1956	Russia	Hungary	.009	.012	.066	-.048	.509	.087

Interventions

1828	France	Turkey	.328	-.244	.330	-.246	-.959	.354
1832	France	Holland	.689	-.472	.602	-.383	.367	.469
1832	UK	Holland	.791	-.584	.650	-.431	.313	.469
1847	Austria	Papal	1.721	-1.424	1.295	-.564	-.117	.645
1850	UK	Greece	.904	-.737	.590	-.400	.250	.562
1854	UK	Greece	.893	-1.424	.429	-.274	.279	.693
1854	France	Greece	.726	-.530	.415	-.256	.429	.693
1860	France	Italy	.565	-.352	.519	-.302	.359	.430
1886	UK	Greece	.956	-.900	.659	-.462	.099	.495
1886	Austria	Greece	.701	-1.874	1.240	-1.033	-.555	.495
1886	Germany	Greece	.890	-1.768	1.137	-.841	-.436	.495
1886	Russia	Greece	1.097	-1.743	1.129	-1.112	-.324	.495
1886	Italy	Greece	.747	-1.871	1.317	-1.149	-.592	.495
1897	Russia	Greece	1.182	-1.647	1.113	-.850	-.242	.457
1897	Austria	Greece	.755	-1.925	1.375	-1.237	-.607	.457
1897	Germany	Greece	1.216	-1.507	1.093	-.739	-.216	.457
1897	Italy	Greece	.931	-1.885	1.340	-1.102	-.480	.457
1897	France	Greece	1.185	-1.447	1.037	-.761	-.170	.457
1897	UK	Greece	.970	-.964	.702	-.504	.047	.457
1905	Austria	Turkey	.497	-1.770	1.112	-1.194	-.524	.226
1905	Russia	Turkey	.872	-1.548	1.184	-1.014	-.357	.226
1905	UK	Turkey	.826	-.891	.802	-.744	.089	.226
1905	France	Turkey	.688	-1.760	1.133	-1.185	-.403	.226
1905	Italy	Turkey	.503	-2.235	1.133	-1.392	-.633	.226
1918	Russia	Estonia	1.007	-.944	.464	-.294	.061	.667
1918	Russia	UK	-.260	-.009	-.300	.018	.061	.345
1920	UK	Turkey	.840	-.732	.755	-.581	.192	.320
1920	France	Turkey	.849	-.974	.756	-.608	-.024	.320
1920	Italy	Turkey	.783	-.817	.686	-.516	.011	.320
1921	France	Germany	-.206	.054	-.133	.111	-.101	-.007
1921	UK	Germany	.093	-.023	.058	-.064	.062	-.007
1921	Belgium	Germany	-.564	.433	-.492	.457	-.081	-.007
1923	France	Germany	-.209	-.026	-.113	.057	-.148	-.027

APPENDIX 2 (continued)

Year	Initiator	Opponent	$E^i(U_{ij})$	$E^i(U_{ji})$	$E^j(U_{ij})$	$E^j(U_{ji})$	R_i	R_j
1923	Belgium	Germany	-.591	.333	-.478	.394	-.148	-.027
1923	Italy	Greece	.759	-.703	.691	-.498	.054	.278
1938	Germany	Austria	.928	-1.732	1.269	-1.070	-.435	.322
1938	Germany	Czechoslovakia	.286	-1.081	.243	-1.071	-.435	-.460
1939	Germany	Czechoslovakia	.538	-1.043	.133	-1.149	-.205	-.458
1939	Germany	Lithuania	.782	-.985	.799	-.732	-.205	.133
1939	Italy	Albania	.808	-.834	.770	-.746	-.113	.128
1939	Russia	Poland	.397	-.439	.064	-.459	-.041	-.524
1939	Russia	Estonia	.636	-.656	.633	-.645	-.041	-.018
1939	Russia	Latvia	.633	-.668	.617	-.726	-.041	-.188
1939	Russia	Lithuania	.687	-.690	.648	-.614	-.041	.133
1940	Russia	Rumania	.774	-.853	.643	-.974	-.041	-.313
1948	Russia	USA	-1.881	1.460	-2.479	1.132	.417	-.378
1948	Russia	UK	-.104	-.554	-.139	-.442	.417	.028
1948	Russia	France	.399	-.720	.172	-1.002	.417	-.262
Threats								
1821	Russia	Turkey	.409	-.813	.974	-1.132	-1.000	.329
1826	Russia	Turkey	.426	-.206	.413	-.191	.384	.412
1840	France	UK	-.714	.307	-.454	.157	-.160	-.957
1840	France	Germany	-.433	-.535	-1.019	-.775	-.160	-.538
1850	Austria	Germany	-.053	.050	-.079	.075	.048	-.755
1853	Austria	Turkey	1.043	-.486	1.013	-.402	.235	.371
1854	Austria	Russia	-.267	.287	-.269	.288	.329	.321
1856	Germany	Switzerland	1.153	-1.318	.979	-.374	-.293	.679
1860	France	Italy	.565	-.352	.519	-.302	.359	.430
1876	Russia	Turkey	.709	-.991	.805	-.513	-.197	.377
1878	UK	Russia	.175	-.409	-.016	-.635	-.001	-.230
1880	UK	Turkey	.000	.000	.000	.000	.020	-.264
1880	France	Turkey	.147	-.113	-.054	-.595	.230	-.264
1880	Russia	Turkey	.013	.136	.003	-.542	.107	-.264
1880	Germany	Turkey	.303	-.630	.084	-.893	-.004	-.264
1880	Austria	Turkey	-.355	-.911	-.412	-.958	-.216	-.264
1880	Italy	Turkey	-.294	.431	-.591	.040	.268	-.264
1881	Turkey	France	-.439	.531	-.465	.544	.332	.302
1885	UK	Russia	.158	-.271	-.173	-.652	.096	-.320
1890	UK	Portugal	1.257	-1.653	1.138	.840	-.248	.470
1898	UK	Turkey	.658	-.892	.662	-.641	.095	.201
1898	France	Turkey	.677	-1.221	.831	-.847	-.195	.201
1898	Russia	Turkey	.915	-1.577	1.056	-.864	-.204	.201
1898	Italy	Turkey	.291	-1.527	.829	-.983	-.460	.201
1898	UK	France	.103	-.021	-.070	-.256	.095	-.195
1906	UK	Turkey	.745	-.790	.970	-.948	.128	.215
1908	Yugoslavia	Austria	-1.574	1.679	-2.242	1.235	.301	-.452
1911	Germany	France	-.346	-.551	-.795	-.691	-.149	-.638
1911	UK	Germany	-.468	-.252	-.392	-.176	-.207	-.149
1912	Russia	Bulgaria	1.001	-2.165	1.406	-1.111	-.526	.541
1912	Austria	Yugoslavia	1.514	-2.172	1.640	-1.253	-.376	.473
1913	Russia	Turkey	.685	-1.777	1.306	-1.247	-.514	.200
1913	Austria	Yugoslavia	1.499	-2.153	1.587	-1.095	-.391	.546
1921	France	Germany	-.206	.054	-.133	.111	-.101	-.007
1921	UK	Germany	.093	-.023	.058	-.064	.062	-.007
1921	Belgium	Germany	-.564	.433	-.492	.457	-.081	-.007
1921	Italy	Germany	-.278	.248	-.264	.256	-.026	-.007
1922	UK	Greece	.770	-.775	.680	-.586	.109	.299
1922	France	Greece	.865	-.141	.839	-.701	-.120	.299
1922	Italy	Greece	.720	-.694	.650	-.450	.024	.299
1934	Italy	Germany	-.686	.373	-.482	.226	-.361	-.754
1934	Italy	Albania	1.214	-1.430	1.287	-1.362	-.361	.033
1940	France	Russia	-.393	.177	-.355	.291	-.425	-.041
1940	UK	Russia	-.475	.419	-.691	.332	.181	-.041

APPENDIX 2 (continued)

Year	Initiator	Opponent	$E^I(U_{ij})$	$E^I(U_{ji})$	$E^J(U_{ij})$	$E^J(U_{ji})$	R_i	R_j
1956	Russia	UK	-.224	1.216	-1.628	-.297	.509	-.726
1956	Russia	France	.379	.345	-.949	-.727	.509	-.809
1956	Russia	Poland	.003	.022	.044	-.021	.509	.179
1957	Russia	Turkey	1.634	-1.401	.201	-3.118	.516	-.725
1957	USA	Russia	-28.864	-3.244	-26.601	-3.382	1.000	.516
1960	Russia	Turkey	1.666	-1.473	.177	-3.169	.480	-.716
1960	Russia	Norway	1.404	-1.648	-.035	-1.879	.480	-.830

The Transmission of Legal Precedent: A Study of State Supreme Courts

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In the course of making and justifying decisions, judges on state supreme courts often rely on precedents from other jurisdictions. These judicial references across boundaries constitute at least one means of communication and, in turn, demonstrate a complex web of deference and derogation between and among various courts. I attempt to uncover patterns of citation between the several state supreme courts and to evaluate alternative explanations for these patterns, including distance between courts; similarity of political culture; the prestige, professionalism, legal capital, and caseload of the cited court; the social diversity of the environment; differentials between courts on a number of dimensions; and presence in the same legal reporting region. More globally, I ask: Does the intensity of communications between a pair of courts result from the characteristics of the cited court or from differences and similarities between courts or jurisdictions? The results indicate the importance of legal reporting districts, distance between the courts, cultural linkages between the jurisdictions and, especially, characteristics of the cited court.

When they are obliged to present rationales for choices among alternative public policies, state supreme courts can and often do cite a plethora of legal, moral, and political authorities. Quite apart from statutes, regulations, constitutions, and their own decisions, these courts of last resort sometimes survey jurisdictions around the nation for relevant precedents.¹ Sir George Cornwall Lewis, who contended that a "large proportion of

the general opinions of mankind are derived merely from authority, and are entertained without any distinct understanding of the evidence on which they rest, or the argumentative grounds by which they are supported" (1875, p. 5), no doubt exaggerated, but he pinpointed a universal human tendency to lean on decisions taken in other times and places. Over a decade ago, Walker (1969, 1971) demonstrated rather clearly that legislatures take cues from several national and regional "leaders," and more recently Canon and Baum (1981) explored a similar phenomenon among appellate courts in the law of torts (see also Grupp & Richards, 1975; Kingdon, 1981; Matthews & Stimson, 1975). Decision makers on appellate courts, probably no less than other political actors and institutions, look for and use the experience of the legal reporters of the high courts of the nation (see Caldeira, 1983). That search stems from not only the very real need for information in the face of high levels of uncertainty (Cyert & March, 1963; March & Simon, 1958), but also the pervasiveness of precedent, regardless of source, as a norm of judicial choice making in the United States.

Judicial references across state boundaries, I contend, constitute at least one dimension of communication and demonstrate a complex web of deference and derogation between and among the various courts.² Yet despite the self-evident

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¹For students of the Supreme Court, Mr. Justice Frankfurter's polls of other jurisdictions, especially in cases involving federalism, remain legendary. Thus, for instance, in *Wolf v. Colorado* (338 U.S. 25 [1949]), Justice Frankfurter, for the Court, remarked that when "we find that in fact most of the English-speaking world does not regard as vital to such protection [the right of privacy] the exclusion of evidence thus obtained, we must hesitate to treat this remedy as an essential ingredient of the right. The contrariety of views of the States is particularly impressive in view of the careful consideration which they have given the problem in the light of the *Weeks* decision." He then proceeded to summarize the actions taken in the several states. See also *Betts v. Brady* (316 U.S. 455 [1942]) for Mr. Justice Roberts' poll of the states.

²I, of course, do not wish to imply that citation of precedent from other jurisdictions constitutes the only or perhaps the most important form of inter-court communication. Quite apart from the "exchange" of precedents, state supreme courts communicate through

importance and intrinsic interest of such phenomena, we have little or no precise conception of the forces that lead courts and judges to prefer the precedents of one jurisdiction over another (cf. Caldeira, 1983; Harris, 1980). In this article, which is based on data on inter-jurisdictional citational practices, I explore alternative explanations of the strength of linkages of communication between the several state supreme courts. More specifically, using all interstate references for all states and the District of Columbia for calendar year 1975, I estimate the relative efficacy of two global accounts—characteristics of cited courts (the *attribute model*) and traits of the relationships between judiciaries (the *relational model*)—for patterns of communication between state supreme courts.

Following the path of Walker (1969), Canon and Baum (1981), and others, this investigation should provide further illumination into the diffusion of policies and practices among political institutions. The bases of judicial communication uncovered here can, in addition, bring us to a clearer or at least fuller appreciation of political leadership among state supreme courts in particular and perhaps even of the more general phenomenon of relations between leaders and followers. In this latter sense, I see this article as a contribution to the burgeoning scholarship on the analysis of networks and social contexts (see, e.g., Knoke & Kuklinski, 1982). Lest anyone be mistaken, flows of political information, such as conversations among the citizenry and cue-receiving and cue-sending inside the legislature, can and often do have quite dramatic consequences for public policy (on mass behavior, see Eulau, 1980; on legislatures, see Kingdon, 1981; Matthews & Stimson, 1975).

forums such as the Conference of Chief Justices, New York University's Institute of Judicial Administration, and the various meetings of bar associations. Furthermore, I acknowledge at the outset that the citation of precedent from other courts can result from motives other than the earnest search for solutions of possible help used in other states. Thus, for example, justices and law clerks might simply string together references on the hope that sheer numbers will impress the consumers of their opinions. Nevertheless, I have encountered no evidence in the data presented here that justices of state supreme courts show a tendency to include the precedents of a particular court or courts more than others in so-called "string-citations"—a bias which, if it existed, would constitute a serious worry.

To lessen the repetition of "inter-state communication," I have taken the literary liberty of using a number of terms. The price, possible confusion about my purposes, pales in comparison to the potential tedium.

Studying Judicial Interactions

Several studies bear in various degrees on the research on interactions between courts I report in this article. Shapiro (1970) has argued that the evolution of the law of torts illustrates the phenomenon that 52 appellate courts which make decisions quite independently of one another arrive at a relatively unified set of doctrines. The principle of comity governs American law, but state supreme courts "constantly cite the decisions of others as persuasive or illustrative or worthy of consideration in making their own" (Shapiro, 1970, p. 51). These patterns in the development of the law of torts provide ammunition for Shapiro's contention that at least in some fields courts constitute very effective networks for the communication of information about the success and failure of policies (Shapiro, 1965, 1970, 1972). More recently, Canon and Baum (1981; see also Baum & Canon, 1982), in a study of innovations in the law of torts, have shown rather persuasively that a particular set of state supreme courts does not lead others in a consistent fashion across doctrines. Innovative judicial policymaking, at least in torts, embraces an appreciable amount of serendipity—not least of all because courts do not take the initiative. Building upon a line of reasoning that spans the intellectual traditions of law and economics, Landes and Posner (1976, pp. 250-251) "treat the body of legal precedents created by judicial decisions in prior periods as a capital stock that yields a flow of information services which depreciates over time as new conditions arise that were not foreseen by the framers of the existing precedents." Most important for my purposes, Landes and Posner articulate the concept of "legal capital"; as we shall see below, that theoretical framework can in a more limited way figure fruitfully in a multivariate model of judicial communication.

Even more central to the present concerns, Harris (1979, p. 1; see also 1980) has argued that "occasions on which [state courts of last resort] put each others' decisions . . . to use are instances of communication between the courts and about the common law." Naturally, Harris asks: "What are the characteristics of the highest appellate courts, and of their jurisdictions, the states, and what are the relations between the states, that have historically led to this communication." On the basis of data from a sample of 16 states collected at decadal intervals from 1870-1970 (see Friedman, Kagan, Cartwright, & Wheeler, 1981; Kagan, Cartwright, Friedman, & Wheeler, 1977, 1978; Mason, 1978), Harris (1979, p. 29) reports that "in all periods, state supreme courts tend to cite the courts of the states from which their own state received its population." In sum, patterns of

migration, capturing the flow of cultural values, account in large part for inter-court communication. Quite apart from traces of migration, state supreme courts' stock of "legal capital" (i.e., the quantity of its reports) figured significantly, although less prominently; physical distance between judiciaries made no difference; presence in the same legal reporting region manifested a moderate coefficient; and, finally, courts located in jurisdictions possessed of laggardly legislatures tended to follow judges in more innovative environments. But even in the context of an impressive multivariate account, the highest appellate courts in New York, California, and Massachusetts garner measurably more references than do others; indeed, in one specification, dummies for these jurisdictions produce coefficients of magnitude second only to migratory patterns.³

More recently but much in the same vein, I have initiated investigations into the transmission of precedent between state supreme courts. These explorations have as a by-product yielded an article on the comparative prestige of the highest appellate courts of the several states (Caldeira, 1983; compare Mott, 1936; see also Merryman, 1954, 1977). In that article I argued that references between courts constitute handy and nonreactive indicators of hierarchies of prestige among the highest judiciaries, and I developed a measure of judicial reputation and examined competing explanations of differentials in prestige. "Social diversity, apart from case load, [ranked] first among the firmament of influences on judicial reputation, and professionalism [trailed] as a distant second" (p. 104). Thus state supreme courts nested in more professional structures and located in socially diverse jurisdictions received, on average, a greater measure of deference. These influences, in the aggregate, account for differences in the sheer amount of references a particular

state supreme court receives, but that does not inform about the relationships between institutions.

That nexus between high benches is the focus of this article.

Issues of Measurement

Through the medium of legal reports, judges on appellate courts leave extraordinarily well-documented clues about processes of decision making. In recent years scholars of public law, now focusing on judicial behavior, have once again begun to follow that set of trails (e.g., Johnson, 1980; Kagan et al., 1977, 1978; see also Cartwright, 1975; Llewellyn, 1960, p. 514). The unit of analysis here—the state supreme court's reference to one of its sister courts—for the purposes of this article seems straightforward enough and did not pose substantial problems of coding and classification. (For a discussion of the vagaries of legal reports as evidence, see Cartwright, 1975, p. 381). Most of the time, a reference across jurisdictions jumped out from the page. (See Nagel, 1962, for evidence that appellate courts almost always cite other jurisdictions in a favorable or neutral fashion.)

The issue, then, is how to operationalize the intensity of the communication between judicial institutions. To encapsulate judicial interaction, I began with a simple count of the number of references each state supreme court made to every other state court of last resort in 1975. Because in many ways it functions as a state, I included the Court of Appeals for the District of Columbia. From that count, I created a large sociometric matrix (51 by 51), which runs the gamut of 2601 relationships. The first row shows the number of times that Alabama's Supreme Court cited those of Alaska, Arizona, Arkansas, etc.; the second row gives Alaska's references to Alabama, Arizona, Arkansas, etc.; and in the last row, Wyoming's mentioned decisions from Alabama, Alaska, Arizona, etc. Some courts make extensive use of precedents from other jurisdictions; others, for a variety of reasons, choose to rely on their own stock of legal rules. Left unstandardized, the matrix is in some circumstances at the mercy of the heaviest users of precedent; such states might well have a disproportionate and misleading influence on the final results.

To standardize for heavy usage, I counted the number of citations each of the courts made in 1975. That in turn facilitated computation of the percentage of this total that any particular supreme court received. If state supreme courts sought legal authority on a random basis, each high bench should receive about 2% of the references from the others. In the summary, each state supreme court should, randomly, have a

³Although Harris's study contains much to recommend it, he bases his generalizations on an exceedingly small sample (16 states)—chosen for good reasons, to be sure, but nonetheless not representative of the full panoply of American states. More serious for a study of relations between governmental units, Harris has observations for only a limited segment of the entire network of such phenomena—a matrix of 16 by 48 instead of the full complement of 50 by 50. Unfortunately, because he must work with a partial matrix, Harris can speak of fewer than a thousand relations rather than a full range of possibilities. Because his colleagues on Yale's project on state supreme courts made a conscious and necessary decision to collect and code only a sample for each of the 16 states, Harris confronted a matrix much less dense in connections than he might have. Consequently, I suspect, he has missed some of the weaker relationships.

total of 100%—2% from each of the remaining 50 high courts. But courts do not use precedent from out of state in a random pattern. These percentages then replace the row numbers in the 51×51 sociometric matrix. In the larger analysis, each interaction in the matrix becomes a dependent variable $N = 2601$). Because I have not counted a court's references to itself, the first case remains missing; the Alabama Supreme Court's citation of Alaska constitutes the second case, of Arizona the third, and of Wyoming the fifty-first. That pattern continues until Wyoming's Supreme Court interacts with the Wisconsin Supreme Court. (The final case, of course, is missing.)

Procedures for matching judicial interactions and explanatory phenomena follow more or less the same pattern. If an explanation consists of an argument that a characteristic of the state supreme court increases or decreases the references it receives (the *attribute model*), then I attached the value to all cases in which it served as the target. Thus, if the Alabama Supreme Court scored 12 on legal professionalism, I placed this number on every 52d case, beginning with the first. That constitutes the simplest set of influences. For another class of explanations, though, I contend that differences or similarities in the characteristics of each pair of state supreme courts depress or facilitate interactions (the *relational model*). For instance, state supreme courts in less diverse jurisdictions might refer to high benches in the more heterogeneous jurisdictions. Or perhaps communication occurs between judges in similar environments. To generate such relational variables, I have performed simple subtraction on the relevant characteristic between the citing and the cited state supreme courts. These influences can, of course, take on both negative and positive values. If negative, the relational variable indicates that the citing court ranks lower; if positive, the cited court has less of the quality.

Explanations of Judicial Communication

There are numerous and often subtle reasons for interactions between appellate courts, and so it should not surprise that a complete explanatory specification involves a raft of influences. Social scientists in previous work have identified a number of bases of communication, such as proximity, similarity, and prestige. At the most basic level these explanations fall into two categories. I shall not dwell on this matter, because the distinction I make is so well-grounded in the literature of social psychology. First, does the intensity of communication between a pair of courts result from characteristics of the cited state supreme court? This I call the *attribute model*. Second, do

differences and similarities between courts or jurisdictions increase or decrease the rate of interaction between appellate judges? I label this notion the *relational model*. From one perspective, it makes good sense to argue that the traits of a state supreme court or the host jurisdiction increase or decrease the attractiveness of its stock of legal precedents. After all, scholars of sociometry have often demonstrated the substantial influence of wealth, physical appearance, and status on the choice of friends and leaders. And yet one can make a good case that myriad social, political, and economic connections structure the communication of precedent between state supreme courts. The importance of the workplace, neighborhood, and political party as determinants of electoral choice comes immediately to mind as an example.

Sifting through the multitude of explanations and variables poses some awkward problems of presentation; most of the explanations have variants that come under both headings. Thus, for instance, I test the effects of the prestige of the cited court and of the differential in prestige on patterns of communication.

Legal Reporting Region

Since the 1880s, the West Publishing Company of St. Paul, Minnesota, has collected and reported the opinions of the several state supreme courts in seven separate editions, divided more or less into geographic regions. Until quite recently, each of the state supreme courts published its own opinions in an individual report, but, especially in the 1960s, more and more of the states shifted to reliance on a regional edition. These reporting regions should make a difference in patterns of citation for at least three reasons: physical access, the scarce resources of time and energy, and psychological distance. The latter two require little elaboration. Lacking the time and energy to scan all of the state and regional reports, lawyers and judges without a good reason for doing otherwise should peruse the regional edition that incorporates the decisions of their jurisdiction. Beyond that consideration, lawyers and judges are likely to perceive the reports of other regions as less salient, not relevant for use in making decisions.

In a bygone era even appellate judges had access to a relatively small proportion of judicial decisions. Not all or even most members of appellate courts then lived and worked in the state capitol (Adamany, 1969). Located in the countryside, some of these judges may have placed reliance on personal, municipal, and county law libraries, most of which had quite limited collections. Nowadays most of the state supreme courts have substantial law libraries, including books for

the chambers of individual judges. Many practicing lawyers have little access to the best and most extensive repositories of judicial opinions, and since appellate briefs constitute one of the means through which precedents come to the attention of state supreme courts (Marvell, 1978), this limitation may well filter back into the reports. To be sure, most lawyers who actually handle appeals before state supreme courts practice in large firms in metropolitan areas and have easy access to a wide variety of reports. Changes in the nature of appellate litigation and improvements in communication (e.g., Lexis) have no doubt blunted the effects of physical access, although I suspect that it may still play a role in the more isolated regions of the United States.

These considerations lead to the expectation that a state supreme court should on average choose the precedent of courts inside the same reporting region more often than it does others. I have in mind here more than the conventional sense of regionalism (e.g., north versus south); in a surprising number of instances, West has not followed traditional usage in allocating a state to one or another region. Thus, for instance, the *Southwestern Reporter* includes opinions from Arizona, Kentucky, Missouri, Tennessee, and Texas—not at all a familiar combination. Should the legal reporting region manifest a significant impact on judicial communication, one can conclude with considerable confidence that, even apart from the standard meaning of region, presence or absence in the same report makes a difference. To evaluate this proposition, I have used dummy variables, scored 0 for absent and 1 for present, for the seven West reporters—Atlantic (*AT*), Northwestern (*NW*), Northeastern (*NE*), Pacific (*P*), Southeastern (*SE*), Southern (*S*), and Southwestern (*SW*). The relevant equation, which I have estimated with ordinary least squares, produces:

$$\begin{aligned}
 JC = & 1.88 + .98 AT + 1.99 NE + .90 SE \\
 & (38.06) \quad (3.95) \quad (2.98) \quad (1.74) \\
 & + 1.78 S + .56 P + 1.95 NW \\
 & (2.67) \quad (3.14) \quad (5.43) \\
 R^2 = & .026 \quad N = 2550
 \end{aligned}$$

where *JC*, the dependent variable, represents judicial communication; *t*-ratios appear under the appropriate unstandardized regression coefficients; R^2 , the coefficient of multiple determination, monitors the amount of variance explained; and *N* equals the number of cases. To avoid statistical problems, I have excluded the southwestern region, and so the intercept yields the coefficient for that variable.

Not at all surprisingly, legal reporting region alone does not account for a substantial amount of variation (a bit more than 2.6%) in the transmission of precedent between appellate courts. Still, each of the regions musters a significant coefficient, all of which at least meet the criterion of .01 level. State supreme courts, regardless of region, manifest a consistent pattern of citing inside rather than outside their own report. The magnitude of the effect varies considerably across regions—from modest, among the Pacific states ($b = .56$), through substantial, for the members of the northwestern ($b = 1.99$) and northwestern regions ($b = 1.95$). Contrary to what one might reasonably expect, supreme courts in the south do not manifest as great a degree of parochialism as do others, at least in this preliminary cut.

Geographical Proximity

For geographers, physical distance between organizations, individuals, and jurisdictions has long functioned as a chief determinant of patterns of human behavior, but political scientists have for the most part ignored this often crucial facet of politics. The simple accident of adjacency sometimes explains a substantial amount about social life. Political communication, for instance, has a greater chance of happening between neighbors or members of the same workplace than it does between two individuals living in different localities. The probability of interaction increases not only because of physical proximity but also because individuals who dwell next to each other normally share a wide range of traits, such as social class, economic wherewithal, and attachment to the neighborhood.

If judicial behavior conforms to these widely observed social laws, it is equally plausible that, *ceteris paribus*, a state supreme court refers more often to the precedents of its neighbors. Numerous influences undergird and reinforce the impact of geographical proximity. Two of these spring immediately to mind: first, each state has very distinctive qualities about it, but neighboring jurisdictions most often share at least some aspects of social, political, and cultural values. For example, although in many ways they are different, Minnesota and Iowa have relatively similar political cultures; New York and New Jersey suffer from many of the same ethnic and religious cleavages; and North Dakota and South Dakota hold in common a pattern of economic activity.

Second, as the physical and I presume psychological distances between two state supreme courts increase, it becomes less likely that justices will regard one another's precedents as apt. This proposition can assume at least two forms: one can argue for a linear and inverse, or, alternatively,

for a curvilinear and negative relationship between distance and communication. The first is straightforward: one state supreme court's propensity to cite another declines proportionately the greater the distance between them. In the second form there is a recognition that, beyond a particular threshold, distance has less and less impact on communication; in sum, that remoteness decreases communication up to a point and then has little or no effect. For appellate judges in an eastern state, for instance, the decisions of the supreme courts of Colorado and Hawaii perhaps do not seem all that different.

To capture the effect of distance (D) on judicial communication, I use number of miles between each pair of state capitols; the square of D is labelled DSQ . The results of a simple regression indicate that distance does indeed have an inverse impact on communication between courts [$JC = 2.51 (31.98) - .003 (7.87) D; r^2 = .024$; note that the t -ratio appears in parentheses to the right of the appropriate unstandardized coefficient], but the relationship manifests distinctively nonlinear qualities [$JC = 3.02 (25.86) - .001 (8.24) D + .17-07 (5.89) DSQ; R^2 = .037$].

Legal Capital

Legal capital consists of "both the abstract, more or less fact-independent rules, and the existing group of matchable fact situations" (Harris, 1979, p. 2; see Landes & Posner, 1976; Posner, 1973, 1977; Rubin, 1977). It represents the collective and collected wisdom, amassed over decades, of an appellate bench. Broad experience, reflected in a stock of legal capital, no doubt makes the decisions of a state supreme court attractive sources of information (on legislatures, see Walker, 1969). Sheer quantity of experience does not, of course, translate directly into influence; breadth, versatility, and competence make a difference. Yet Shapiro (1972) has underscored the importance of "redundancy" in networks of communication. "Legal discourse," he states, "in the style of *stare decisis*, then, is not a unique phenomenon, but an instance of communication with extremely high levels of redundancy" (1972, p. 129; see also Landes, 1969). The larger the stock of legal precedent state supreme court possesses, the more often it can figure in the opinions of sister courts, regardless of how voluminous the store of cases in the reports of these benches.⁴ Or, alternatively, perhaps a court

possessed of a huge store of precedents does not serve as a favorite for the entire range of supreme courts, but, rather, for benches lacking a substantial amount of doctrine.

To measure legal capital, I have used the number of running feet of legal reports each state supreme court has produced from its inception until 1970 (Merryman, 1977, pp. 401-404; cf. Harris, 1979, p. 5)—although in a more perfect world I should have preferred to have the number of decisions made or of pages written. At the simplest level, it turns out that a sizable legal capital does make a court more central in the network of communication [$JC = 1.25 (18.06) + .017 (14.96) LC; r^2 = .082$]; and that supreme courts relatively low on precedent refer more often to courts with more legal stock [$JC = 2.04 (43.99) - .008 (10.35) LCDIFF; r^2 = .042$].

Size of Caseload

Especially in the last decade and a half, scholars, lawyers, and judges have increased public and professional consciousness of the existence of a crisis in the volume of and demand for legal processes (see Caldeira, 1982; Casper & Posner, 1976). Quite apart from the issue of justice for individuals, the crisis of volume often leaves pronounced marks on the nature of the business of state supreme courts (Kagan et al., 1978, p. 971). For instance, "on average, courts with larger caseloads wrote more opinions which were shorter, which used few citations, and which referred less often to treatises, legal encyclopedias, and law reviews" (Kagan et al., 1978, p. 972). Signs suggest that overload increases the "risk of routine, poorly crafted opinions," not at all a surprising consequence. Opinions dashed off with abandon and based on trivial disputes do not make prime candidates for notice among state supreme courts. Flooded dockets in courts of last resort go hand in hand with the lack of an intermediate appellate bench and discretionary powers of review; if a state supreme court has a set of judges interposed between it and the lower reaches of the judiciary and has the authority to reject cases, it can usually hold its workload to manageable limits. The presence of an intermediate appellate bench, then, increases judicial leisure and discretion. In consequence, the nature of the cases changes, judges become more fractious and opinionated, and courts probably evince greater willingness to take bold in-

⁴Merryman remarks: "Assuming some sort of even, if random, distribution of different fact situations and legal questions among the reported decisions, the probability that one will find a case in point in the decisions of

a given state should be a function of the number of its published decisions. . . . Thus, the more published decisions, the more citations" (1977, p. 403).

itiatives in policymaking (see Atkins & Glick, 1976; Canon & Jaros, 1970; Jaros & Canon, 1972).

Regardless of the causes, on the basis of these considerations, heavy caseloads should decrease the attractiveness of a state supreme court's decisions. For example, the Rhode Island Supreme Court, which issues hordes of opinions almost all utterly lacking originality (Beiser, 1973), receives the notice of very few of its sister courts (Caldeira, n.d.). There are no doubt upper limits on the capacity of excessive caseloads to decrease reliance on a court's precedents; after all, even the hardest-pressed benches figure in at least some of the decisions of state supreme courts around the nation. Preliminary statistical analyses provide some slight evidence that the size of its caseload (taken from Marvell & Kuykendall, 1980) decreases the probability that a state supreme court's precedents will achieve widespread attention [$JC = 2.41 (23.19) - .009 (4.74) C$; $r^2 = .009$] and a shred more that it does so at a diminishing rate of effectiveness [$JC = 2.97 (14.01) - .034 (4.05) C + .0002 (.303) C^2$; $R^2 = .01$].

Judicial Professionalism

Structural characteristics of courts vary a great deal across American state jurisdictions—from the simple, streamlined, and modern through the complex, variegated, and traditional. Professionalism, some have argued, implies a modernized structure and an absence of partisan politics in the selection and retention of judges. Legal reformers have for many years, and with substantial success, attempted to gain public support for an ideal of state judiciaries as separate, hermetically sealed off from the nefarious influences of parties, governmental officials, and the siren song of public opinion. This conception in fact usually translates into the adoption of the American Bar Association's models for judicial selection and organization, creation of an office of professional administrator of the courts, more or less permanent tenure for judges, and, finally, salaries munificent enough to attract and retain jurists of high stature (Glick & Vines, 1972, p. 11).

For the most part, more professionalized judiciaries can claim greater prestige than less professionalized state supreme courts (Caldeira, 1983, p. 99). This should be expected, since it is the lawyers and judges who have set out the desiderata of professional judiciaries. These professional courts, commanding high respect and having marked out a trail of independence, may well find it easier to recruit judges of remarkable abilities. Insulated in the main from considerations of partisan manipulation, such jurists may "produce decisions that are more widely ap-

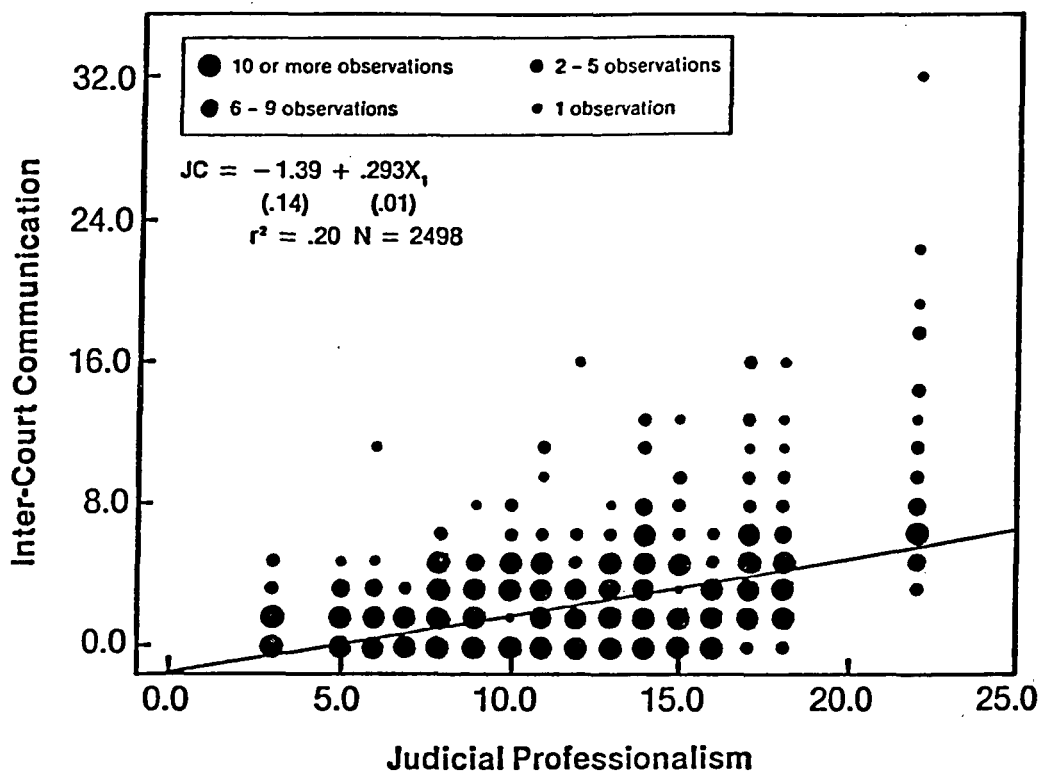
plicable over states and time, because they can afford to let abstract rules rather than political concerns decide cases" (Harris, 1979, p. 10). In sum, if members of a state supreme court need not fret about the reaction of this or that faction or cabal to particular facets of its decisions, then these judges have the freedom to make more principled choices. Such precedents presumably have a better chance of serving large numbers of jurisdictions.

Law professors, who played a large part in creation of the American Bar Association's criteria, no doubt accord the professional courts quite favorable treatment in scholarly circles such as law reviews, casebooks, encyclopedias, and perhaps most important of all, the classrooms of schools of law around the nation. It turns out that the relationship between professionalism and prestige is quite strong (Caldeira, 1983, p. 99). Some fifty years ago, Mott (1936, p. 305) reported a very high correlation ($r = .89$) between a court's prestige, monitored through other bench's references to it, and the number of its opinions reprinted in a sample of casebooks. He observed, "It is clear that those courts which are drawn on most frequently for opinions in the casebooks are more likely to influence the legal thinking of the next generation of lawyers than those which are ignored" (p. 305). Moreover, as I remarked earlier, an intermediate appellate court, which is an important component of professionalism, allows a state supreme court the freedom necessary for repose, reflection, discussion, and writing (see Atkins & Glick, 1976; Canon & Jaros, 1970; Jaros & Canon, 1972; Kagan et al., 1978, p. 972).

These considerations compel the proposition that state supreme courts more often invoke the decisions of high courts high on the scale of professionalism. It is equally plausible, however, that differences in professionalism between courts color patterns of communication. Thus, a bench low on professionalism might refer often to the set of courts highest on that attribute, but courts located in more or less similar ranges of professionalism—high or low—might not choose each others' precedents. To measure professionalism, I have chosen a straightforward and often-used indicator (Glick & Vines, 1972; for applications, see Canon & Baum, 1981; Glick, 1981) that has a potential range of 0 through 25 and an actual of 3.4 through 21.7.

Figure 1, which portrays the simple relationship between professionalism and judicial communication, supplies substantial support for the proposition at issue: state supreme courts ranking high on professionalism achieve more attention. Still, as befits a relationship at such a level of aggregation, there is a great deal of dispersion around the line

Figure 1. The Effect of Judicial Professionalism on Communication Between Courts
(Figures in parentheses are standard errors.)



Sources: The data on professionalism are from Glick and Vines (1973, p. 12). The data on communication were collected by the author.

of regression. Finally, differences between states in professionalism do matter: courts low on the totem pole, on average, more often choose precedents of the higher-ups [$JC = 2.04$ (45.25) - .141 (16.00) $LPDIFF$; $r^2 = .095$]—although this relationship is not as strong as that for the initial formulation.

Social Complexity

High levels of social diversity and development create an environment particularly hospitable to the use of law and to doctrinal innovation. "In societies in which the level of economic development is relatively low and in which social relationships are close and interdependent, customary devices of social control and mechanisms of conflict resolution seem to predominate" (Grossman & Sarat, 1975, p. 323; see also Schwartz & Miller, 1963). These ties that bind, once broken, give way

to an explosion of complex disputes, new varieties of rights claimed, and, of course, room to spare for creative common-law judging. Social change instigates problems that soon make the application of tried-and-true legal rules anomalous, so judges especially on state courts of appeal must reshape old doctrines and invent new ones. Large populations translate into more cases at law (for a theory of litigation, see Casper & Posner, 1976; cf. McIntosh, 1983) and, in turn, into more and more "problematic" issues (see also Dahl & Tufte, 1973). Conversely, "sparse population reduces opportunities for judicial innovation" (Canon & Baum, 1981, p. 980). Industrialization and urbanization, which almost always occur at the same juncture, increase the use of law and litigation (Friedman, 1973, p. 17). There is some evidence that litigation, if not the use of law per se, tapers off as industrialization, population, and urbanization reach a threshold; but a preponder-

ance of the evidence supports the view that these forces constitute leading indicators during an initial period.

Regardless of the precise character of the relationship, we know that if and when a new issue comes to the fore, it most often arises in the most populous, diverse, industrialized localities. State supreme courts at work in more diverse milieus, processing more cases of first impression, have the opportunity to contrive precedents that, if not always adopted intact, at the very least merit the rapt attention of jurists who follow in the wake. Or, in an alternative formulation of the relationship, supreme courts in the least modern should invoke the previous decisions of the benches in the more modern states; in essence, the parochials should follow the cosmopolitans. To operationalize societal complexity, I have chosen the size of a state's population—not an ideal measure.⁵ The

⁵There are a number of paths I could have taken in testing the notion that social complexity influences interstate communication. First, as I mentioned in the text, I could have divided the general idea into a series of more specific propositions about the effects of urbanization, industrialization, size of population, and growth on patterns of citation. Lamentably, because of the rather substantial correlations among these variables, statistical problems immediately arise. Thus, for example, depending upon how I defined urbanization, the correlation between it and population varied between .5 and .7. If I included indicators of industrialization, population, change in population, and urbanization in the same equation, multicollinearity caused some of the signs to run in a nonsensical direction. It is, for instance, implausible that urbanization decreases the attractiveness of a state supreme court's precedent, but that is exactly what I encountered in equations with industrialization, population, and urbanization. Furthermore, if in some formulations the signs for urbanization or industrialization did not go awry, the coefficients always approached zero. The size of a state's population washed out the effects of other variables, and this result held regardless of how I operationalized urbanization or industrialization. Moreover, the inclusion of variables other than size of population neither improves the statistical fit nor changes the substantive results.

Second, I could have constructed a composite of a number of important indicators of societal complexity, a strategy that many students of comparative politics have adopted. In the context of American subnational politics, some scholars have used techniques of data reduction such as factor analysis to transform larger numbers of variables into a smaller set of dimensions. Sharkansky and Hofferbert (1969), for example, reduced a passel of indicators into factors labelled "affluence" and "industrialization." These composites, though tidy, naturally lack conceptual clarity and an intuitively discernible metric. The items that constitute "industrialization," for instance, are a disparate lot, ranging from the value of industrial property to the size of population. A single title, "industrialization," im-

plies a good deal more coherence than indeed exists. If a significant relationship between judicial communication and a composite of "industrialization" or "affluence" had materialized, I am not at all certain how I could or should have interpreted it.

correlations among indicators of urbanization, size of population, and industrialization run so large that one cannot distinguish among them. That fact is a double-edged sword; it means, on one hand, that I cannot estimate the separate effects of the three forces on inter-state communication, but on the other hand, because size of population dominates, I can treat it as a proxy for the constellation of forces parading under the rubric of modernization. In any event, statistical analysis yields solid evidence of the impact of societal complexity on patterns of communication; population alone explains 30% of the variation in rates of reference [$JC = .829 (15.38) + .296-07 (32.47) P; r^2 = .293$]. Furthermore, the differential in population for a pair of states has a pronounced effect on the interaction between the two supreme courts [$JC = 2.01 (46.94) - .15-07 (20.81) PDIFF; r^2 = .145$], although, as in foregoing explanations, this more sophisticated formulation stands at least initially as less compelling than the simpler one.

Prestige

The relative prestige of a particular supreme court might incline the judges of other courts to consult and to invoke its precedents more often and to lend them more weight than these judges might otherwise do (see Mott, 1936).⁶ Evaluations regarding prestige "are functionally autonomous from the hierarchies of wealth and power—that is, . . . subjective responses to particular social positions that are rooted in generally shared" values (Laumann & Heinz, 1977, p. 161). Thus a supreme court might, because of the influence of prestige, receive more than the share of references predicted simply on the basis of other considerations. For instance, in 1975 state courts of last resort used the California Supreme Court's

plies a good deal more coherence than indeed exists. If a significant relationship between judicial communication and a composite of "industrialization" or "affluence" had materialized, I am not at all certain how I could or should have interpreted it.

In light of these issues, I chose the size of population as an indicator of social complexity. It does not, I admit, perfectly reflect all aspects of social complexity. But the size of population surely constitutes one of the most crucial characteristics of this phenomenon. Having opted for one indicator with a clearing meaning and metric, I know what I have and do not have.

"Goldthorpe and Hope define prestige as a "particular form of social power and advantage that is of a symbolic rather than an economic or political character, and which gives rise to structured relationships of deference, acceptance, and derogation" (1972, p. 19, 21).

precedents far more often than the size of its population, the extensiveness of its legal capital, or its rank on judicial professionalism indicated (Caldeira, 1983).

In this article, Harris specifies dummies for the high courts of New York, California, and Massachusetts, because these benches "have historically had a great deal of prestige" and in the analyses become "statistical outliers" (1979, p. 18). Indeed he reports particularly large coefficients for the highest benches of Massachusetts and New York in the period between 1870 and 1900 and for California after World War II (Harris, 1979, p. 27a). So far I have reviewed the argument for and evidence of the simple, linear effect of prestige on intercourt communications; that line of reasoning implies that all state supreme courts choose the precedents of the more prestigious over the less prestigious. It is equally possible that the least prestigious state supreme courts might invoke the common law of the most, and the highest ranking might opt for their own stock of decisions. This in effect means that the differences between courts and not the characteristics of the cited high bench affect rates of interaction. If such a pattern occurs, we should observe a strong association between communication and the differential in prestige between courts.

To test this pair of propositions, I use a variation on a summary indicator of the total references each state supreme court drew from others as a measure of prestige. To guard against spurious associations, I have purged these two indicators of any artifactual correlations between prestige and judicial communication.⁷ This route

is preferable because it allows us to use a high level of measurement and just one variable. These relationships, at least at the simplest level, make a very strong showing: the most prestigious supreme courts obtained more references than did the least [$JC = 2.01 (51.64) + 1.27 (32.45) PREST; r^2 = .29$], quite an impressive fit for data of this sort, and benches lowest in the hierarchy manifested a marked tendency to opt for the precedent of the highest [$JC = 2.007 (46.98) - .628 (20.96) PRESTDIF; r^2 = .147$].

Cultural Linkage

Social scientists have demonstrated over and over again the effects of the social, political, and economic similarities among states (Savage, 1978; Sharkansky, 1970) and of the regional loyalties and affiliations (Gray, 1973; Walker, 1969) on the making and contours of public policy. These shared values or "political culture," it is thought, predispose states to reach decisions of more or less the same character. Too often, of course, political culture has functioned as an explanation of last resort, an account of any and all inexplicable residual patterns; but, although abused, this line of reasoning is appealing, for it squares with daily observation. No one, for example, doubts the distinctive nature of southern politics and culture. Shared cultural values—political, social, and economic—stem from patterns and flows of migration and settlement in the United States. As populations of the various states have moved across the nation and found new homes, these individuals have carried with them and transplanted into new soil the attitudes, beliefs, and practices of a former life. The leading exponent of this line of thinking (Elazar, 1966, 1970) contends that

⁷Conceptually, as I have argued in the text, I believe that we can distinguish between prestige and inter-court communication—just as we separate wealth, political power, and prestige. In fact, the idea is quite simple: a state supreme court is more likely to cite another court if others also refer to it often. Empirically, I would of course have preferred to use an attitudinal indicator of justices' perceptions of the prestige of state supreme courts, but that seemed out of the question. Although the measures of inter-court communication and of prestige derive from the same original source—a count of citations in legal reporters—the subsequent transformations performed make them empirically distinguishable indicators.

To operationalize prestige, I begin with the scores from a count of the total of references that any particular supreme court has received from all others. This ranges from 461 for California to 0 for the District of Columbia. Then I attach this score for a court to each case in which that court is on the receiving end of a reference. At this point, for each case, I subtract the dependent variable from the score on prestige in order to purge the latter of any artifactual association with judicial communication. For any particular pair of state

supreme courts (e.g., Alabama cites Colorado), this number is the score on prestige for the receiving court minus the score for the share of the first court's references devoted to the second court. In other words, in computing prestige here, I have excluded references between pairs of state supreme courts involved in each case. To create a differential measure of prestige, I have followed the procedure outlined in the third section.

These transformations apparently succeed, for the statistical analyses manifest no telltale signs of problems. The coefficient of determination, for instance, although substantial, is not overwhelming ($r^2 = .29$). Last but not least and to anticipate the final results, as a precautionary matter I have estimated the multivariate regressions presented in Tables 1-3 without prestige and found that its exclusion had only a modest influence on the size and stability of other coefficients. In these alternative regressions, the only appreciable difference was that the fit was a bit worse and the effect of caseload became more pronounced.

patterns of migration and especially the settling of the frontier had a major hand in shaping the outlines of a state's political culture; succeeding waves in the later nineteenth and early twentieth centuries reinforced and sometimes substantially modified the initial distribution of values (see, more generally, Gastil, 1975a). To the extent that a state furnishes the citizens for another, it has an influence on the culture of that jurisdiction.

If patterns of migration have a profound effect more generally on politics, it is plausible to expect that at least a minimal cultural linkage should increase communication among state supreme courts. If citizens of one state move to a second and if this reasoning is correct, the state supreme court of the second should on average choose from among the stock of legal precedent of the first's highest court more often than from others.⁸ Cultural linkage might translate into higher levels of intercourt communication, first through lawyers, law professors, and litigants who bring with them a preference for the precedents of their former homes, and second, through judicial favoritism toward the law of a particular state seen as somehow similar or as an appropriate leading light in the region.

In sum, if a state contains an appreciable number of citizens from another jurisdiction, so the argument goes, its supreme court should choose the precedents of that state's high bench more often than it does others. To measure cultural linkage, I have used data on migration—more specifically, Gastil's estimates of the proportion of a particular state's population drawn from the several states (1975b). In the results of a simple regression, there is a substantial amount of support for the idea that patterns of migration influence the character and extent of communication among state supreme courts [$JC = 1.63 (35.76) + .709 (22.80) M; r^2 = .17$].

Findings

Thus far, I have presented some preliminary evidence for and against several explanations for patterns of interstate communication of precedent, and, for all of them, at least some support has materialized. The issue that remains, then, is: Does each of the potential determinants exercise

an independent and significant influence on intercourt communication? In outlining these explanations, I have at various junctures spoken of two global categories—relationships between states, and characteristics of the cited court. Before evaluating the fully specified equation, I present statistical results first of all for the relational model, consisting of differentials in various attributes, proximity, legal reporting region, and the distribution of migration, and second, for the attribute model, which comprises particular characteristics of cited courts. The results so far indicate more support for the idea that the traits of state supreme courts and their contexts facilitate the communication of precedent more than the relationships between pairs of benches. I shall show in a moment how that notion fares. Table 1 embodies estimates from a regression of patterns of communication on relational influences. Recall that, aside from distance, legal reporting region, and cultural linkage, each independent variable is the result of subtracting the cited supreme court's value from the citing court's.

First of all, taken as a whole, this equation yields a decent first step toward an explanation of intercourt communication; the fit is adequate, if not impressive; 12 of the 13 coefficients reach .15 level of significance and most do much better, and all but one of the signs run in the expected direction. These results indicate that, in general, differentials between state supreme courts do indeed influence patterns of communication. If a state supreme court has relatively little prestige and ranks low in professionalism and social diversity, on average, such a high bench more often invokes the precedents of its sister courts higher up the ladder. Furthermore, cultural linkage manifested a substantial impact on judicial interactions; each 4% increase in the proportion of a state's residents who have migrated from another jurisdiction increments by 2% the share of the state supreme court's precedents drawn from the former home of its new inhabitants. Distance between state supreme courts performs exactly as expected. Remoteness decreases communication until a threshold is reached and then exercises less and less effect, but before that threshold is reached, each thousand miles of territory between a pair of courts depresses the proportion of precedents invoked by about 0.33%.

Legal capital, on the other hand, has an effect quite the opposite of that which I had suggested. Supreme courts rich in legal capital apparently opt for the decisions of high benches less well-stocked—instead of, as I argued, the other way around. The California Supreme Court's dominance of intercourt communications (Caldeira, n.d.) no doubt does much to reverse the predicted relationship, since the Golden State joined the

⁸In Nevada, for example, much of the population has come from next door in California. The Nevada Supreme Court, in choosing among the decisions of state appellate courts, has invoked the precedents of California's Supreme Court in about 32% of the instances. No doubt the extraordinary dependency of Nevada on California marks an upper limit, but this stark example sensitizes us to the potential effects of migration on interstate communication.

Union at a relatively late date. Finally, the coefficients for West's regions support in general the proposition that the manner in which courts disseminate their opinions has an impact on communication, although the results here are not so unequivocal as in the bivariate setting.

Standardized regression coefficients, which appear in the first column of Table 1, afford a comparative assessment of the importance of the alternative explanations. (By way of a caveat, I should state that interpretations of the standardized coefficients from quadratic formulations because of multicollinearity most often prove hazardous. For this reason, I eschew substantive remarks about the relative influence of distance.) Cultural linkage ranks first and differential in prestige second among the influences on intercourt communication, and societal complexity trails in third place. Furthermore, legal capital and judicial professionalism, although not nearly so potent as cultural linkage and prestige, nevertheless manifest a measurable impact. Of the several state supreme courts, judiciaries in the Northwest, Northeast, and Atlantic regions of West's scheme demonstrate a rather pronounced bias in favor of precedent within their own reports. Still, alongside the results for prestige and cultural linkage, the standardized coefficients for legal regions pale in comparison.

There is, however, equally and in some ways more persuasive evidence in support of the contention that the characteristics of the cited court structure the communication of precedent—as Table 2 suggests. Once again, we encounter significant coefficients in abundance, signs for the

most part in the correct direction, and an even better fit. These coefficients imply that prestige, professionalism, and societal complexity increase the chances that a supreme court's precedent will accumulate references. Here again the errant sign for legal capital appears, and, again, as an explanation, I submit the influence of California, a court relatively poor in legal capital. If a supreme court processed a large caseload, *ceteris paribus*, its sister courts invoked its decisions more often than if it had a smaller one, but of course after a certain juncture size made less and less difference. Of the independent variables, societal complexity and prestige exert the greatest influence, and professionalism and legal capital significantly less. In this pass at the data, the large and prestigious states collect the lion's share of the references. Table 3 presents the estimates for the fully embellished equation and allows us to speak in more authoritative terms. A more complete assessment yields some coefficients at odds with the results reported in Tables 1 and 2. These appear because the full range of controls unmask several apparent but ultimately spurious relationships.

Perhaps most striking, four of the most plausible relational characteristics fail to reach or approach statistical significance in this fuller consideration; differentials between states and courts on the dimensions of prestige, professionalism, societal complexity, and legal capital do not influence the communication of precedent. The fit of this equation is very impressive in light of the inevitable noise present in such data. In the choice of precedent, then, the characteristics of the cited

Table 1. Regression of Intercourt Communication on Relationships between States and Courts

	β	<i>b</i>	<i>t</i> -ratio	Significance
Intercept		2.021	16.31	.001
Prestige	-.232	-.386	-9.10	.001
Legal professionalism	-.110	-.051	-5.39	.001
Size of population	-.179	-7.192-08	-7.06	.001
Legal capital	.138	.006	6.16	.001
Cultural linkage	.324	.571	17.32	.001
Distance	-.132	-.0003	-2.57	.005
Distance squared	.079	3.88-08	1.47	.150
Legal reporting districts				
Atlantic	.046	.557	2.28	.010
Northeast	.047	1.401	2.49	.005
Southeast	-.009	-.222	-.50	n.s.
South	.026	.783	1.39	.150
Pacific	.024	.214	1.42	.150
Northwest	.068	1.174	3.82	.001
R^2	.324			
<i>N</i>	2450			
<i>D.F.</i>	2436			

Table 2. Regression of Intercourt Communication on Characteristics of Cited Courts

	β	b	t -Ratio	Significance
Intercept		-.337	-1.11	n.s.
Prestige	.277	.653	10.16	.001
Legal professionalism	.154	.101	7.47	.001
Size of population	.363	2.099-07	14.07	.001
Legal capital	-.119	-.007	-5.80	.001
Caseload	.260	.026	3.26	.001
Caseload squared	-.241	-2.11-04	-3.03	.001
$R^2 = .38$				
$N = 2450$				
$D.F. = 2443$				

court dominate. Thus a supreme court's prestige, professionalism, societal diversity, and, to a lesser extent, the size of caseload can and do incline the highest courts of the several states to invoke its decisions more often. Legal reporting edition, too, intervenes in the transmission of precedent. Supreme courts within the same region, aside from the northeasterners, manifest a significant propensity to choose each others' decisions. That phenomenon comes out most clearly for the Northwest and the Pacific and least for Northeast and the Southeast.

Although differentials for the greater part wash out, two—distance and patterns of migration—remain central in a full-blown account of judicial communication. The logic is simple: the greater the distance between two supreme courts, the lower the likelihood of communication, and the more substantial the cultural penetration of one state by another, the more likely the recipient state court is to cite the precedents of the original name state's court. On the basis of the standardized coefficients, prestige and societal complexity rank first in consequences; cultural linkage and legal capital fall in a second stratum. Legal professionalism exerts just a little less influence. It is unfortunate that for statistical reasons I cannot make much of the coefficients for caseload and distance, for on both intuitive and theoretical grounds this pair of explanations holds promise. That much said, logarithmic rather than quadratic versions produce quite an impressive standardized coefficient for distance and a miniscule one for the size of caseload. For the natural log of distance, $\beta = -.209$; for a similar formulation of caseload, $\beta = .05$. Finally, although none of standardized coefficients for legal reporting regions is in and of itself very large, these dummies taken together translate into a substantial influence on the way appellate judges choose precedents. Decisions made at the West Publishing Company years ago have had marked consequences for judicial communication.

This full model explains about half of the variance in patterns of communication, but much remains. Residuals often shed light on the weaknesses of an explanatory scheme, so for each of the cited supreme courts I have computed a mean citational score (i.e., the average share of references received) and mean residual (i.e., the average difference between predicted and actual for the sum of the interactions). These results (not shown) exhibit no systematic patterns of error—that is, the state supreme courts for which the model produces the largest errors share no obvious characteristics or relationships. The California Supreme Court so dominates the communication of precedent that even after a full multivariate treatment, it leads the pack. More interesting, some unexpected high benches generate large positive residuals: the model substantially underestimated the shares of the supreme courts of Alabama, Kentucky, Mississippi, Missouri, South Dakota, and Wyoming. These supreme courts do better in gathering citations than predicted on the basis of such factors as prestige, societal complexity, migration, and distance. Equally surprising, the supreme courts of Connecticut, Michigan, Ohio, Oklahoma, and Texas did much less well than the average high bench. It is not, of course, that these courts receive few references, but rather that they score so high on most of the independent variables and in consequence build up unfulfilled expectations.

Summary and Conclusion

Characterized as they are by so many nuances, patterns of reference between state supreme courts defy simple description or explanation, but in this article I have attempted to uncover these traces of intercourt communication, and, more important, to evaluate alternative explanations of such forms of communication. These rival accounts include distance between courts, similarity in the political culture, the prestige, legal struc-

Table 3. The Full Equation for Intercourt Communication

	β	b	t -Ratio	Significance
Intercept		-.091	-.27	n.s.
Characteristics of cited court				
Prestige	.255	.601	7.14	.001
Legal professionalism	.192	.126	6.04	.001
Size of population	.363	2.04-07	10.32	.001
Legal capital	-.153	-.009	-5.77	.001
Caseload	.220	.022	2.97	.005
Caseload squared	-.184	-1.64-04	-2.54	.005
Relational characteristics				
Prestige	-.026	-.043	-.79	n.s.
Legal professionalism	-.024	-.011	-.87	n.s.
Size of population	.003	1.175-08	.91	n.s.
Legal capital	.005	.0002	.13	n.s.
Cultural linkage	.213	.375	12.64	.001
Distance	-.307	-.0007	-5.82	.001
Distance squared	.198	1.039-07	4.43	.001
Legal reporting districts				
Atlantic	.055	.678	3.04	.001
Northeast	.011	.342	.70	n.s.
Southeast	.024	.573	1.49	.150
South	.049	1.493	3.01	.005
Pacific	.084	.745	5.25	.001
Northwest	.078	1.348	4.66	.001

$R^2 = .490$
 $N = 2401$
 $D.F. = 2382$

ture, legal capital, and caseload of a court, the societal complexity of a state, and presence in the same legal reporting district. Based on analyses of all citations between state supreme courts in 1975, the results indicate the importance of legal reporting districts, distance between the courts, cultural linkages between the states, and most fundamentally the characteristics of the cited court (e.g., prestige). Statistical analyses of single elements of the full model proved misleading; if I had halted after examination of the results on the relational explanations, I would have arrived at very different—and wrong—conclusions.

More globally, although there is substantial support for both conceptions, the attribute model on the whole outperformed the relational model of judicial communication. Social connections—ties that bind state supreme courts—do indeed matter a great deal, but it is a court's characteristics that contribute most to the attractiveness of its legal precedents. The success of the attribute model in this context places this form of communication between courts in the same company as many other formal and informal institutions or groups.

The results I have reported here bring into vivid

relief the broad similarities between the transmission of legal precedent among state supreme courts and the flow of information and cue-taking within and between legislatures. Legislators, we know, lean on like-minded and similarly situated fellow specialists. Innovative policies apparently diffuse in quite different fashions among legislatures and courts (Canon & Baum, 1981), but the research I have reported here, insofar as it contributes to the comparative analysis of institutions, establishes the tendency of state supreme courts to choose the precedent of "leaders" on a variety of dimensions. These judges, like their colleagues in legislative halls, evince a marked propensity to rely on the lead of the more professional, prestigious courts located in the more diverse and populous states. Legal precedent flows from the more diverse, prestigious, and professional to the state supreme courts that rank lower as well as high on these dimensions.

To the extent that these leaders consistently adopt policies of a certain ideological stripe, the flow of precedent introduces a potentially important bias in the network of communication. Unfortunately, there is no reliable, quantitative measure of the ideological content of state

supreme court's decisions, but it takes no more than a cursory examination to bring one to the preliminary conclusion that the courts cited most often in most jurisdictions have a decidedly liberal cast. If closer analysis supports this speculation about the political character of the state supreme court's decisions, then there is good reason to believe that policies and precedents of a particular kind will more often receive the attention of appellate judges. In addition, it is most likely that the sorts of precedent that state supreme courts will choose will, for example, favor criminal defendants, tenants over landlords, debtors over creditors, and plaintiffs against defendants in tort. Of course from another perspective it makes perfect sense that the decisions of the more liberal state supreme courts receive greater notice. These courts, after all, demonstrate a greater willingness to move away from the status quo. Precedents in which a court announces a change naturally cause more comment and controversy than does an affirmation of past practice. Still, whether natural or not, such a bias in the content of the flow of precedents does exist.

So far I have emphasized the vertical bias of communication between courts, but I suppose that the mere existence and the pervasiveness of such flows of information between state supreme courts testify to the health and vigor of our federal system. National political institutions may dominate much of the time in most fields of endeavor, but in the development of the common law, state supreme courts dance to their own drummer. To be sure, because of the biases in the network, the tune is likely to have national sound about it. In spite of the bias in favor of the precedents of more prestigious, professional courts in diverse states, there are a number of vestiges of a persistent regionalism. Legal reporting regions, cultural affinities, and distance constrain the flow of precedent. Here as elsewhere (see Walker, 1969), such forces stand as barriers to the complete homogenization and nationalization of policies across jurisdictions. The interesting issue of whether the influence of regional forces in the diffusion of precedent has declined as media of communication have improved—a plausible hypothesis—lies outside the purview of this study.

Quite apart from the light shed on the transmission of legal precedent, the results I report here have implications too numerous to mention for the study of political communication within and between organizations and the relationship between information and decision making. To note just two, though, the search for information within state supreme courts takes place vertically more often than horizontally and is to a surprising degree quite limited—which makes the courts,

especially in this latter regard, very much the same as most organizations and individuals.

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Forthcoming Articles

The following articles have been scheduled for publication in the June, 1985 issue.

Richard Born, "Partisan Intentions and Election Day Realities in the Congressional Redistricting Process"

Bruce E. Cain, "Assessing the Partisan Effects of Redistricting"

Richard E. Dagger, "Rights, Boundaries, and the Bonds of Community"

Ellen Jones and Fred W. Grupp, "Modernization and Traditionality in a Multiethnic Society: The Soviet Case"

Douglas Madsen, "A Biochemical Property Relating to Power Seeking in Humans"

Eileen L. McDonagh and H. Douglas Price, "Woman Suffrage in the Progressive Era: Patterns of Opposition and Support in Referenda Voting, 1910-1980"

Arthur H. Miller and Martin P. Wattenberg, "Throwing the Rascals Out: Policy and Performance Evaluations of Presidential Candidates, 1952-1980"

Charles W. Ostrom, Jr., and Dennis M. Simon, "Promise and Performance: A Dynamic Model of Presidential Popularity"

Robert D. Plotnick and Richard P. Winters,

"A Politico-Economic Theory of Income Redistribution"

Keith T. Poole and R. Steven Daniels, "Ideology, Party, and Voting in the U.S. Congress, 1959-1980"

Karen A. Rasler and William R. Thompson, "War Making and State Making: Governmental Expenditures, Tax Revenues, and Global Wars"

Herbert A. Simon, "Human Nature in Politics: The Dialogue of Psychology with Political Science"

John R. Wright, "PACs, Contributions, and Roll Calls: An Organizational Perspective"

Errata

In "Equality, Efficiency, and Politics in Soviet Bilingual Education Policy, 1934-1980" by Barbara Anderson and Brian D. Silver (Vol. 78, p. 1031), the graphs for Figures 1 and 2 were inadvertently transposed. The lower graph should be Figure 1; the upper graph belongs with the caption for Figure 2. We regret the error.

In Jack R. Van Der Slik's review of *Computers and Politics* by James Danziger, William Dutton, Rob Kling, and Kenneth Kramer (1983, Vol. 77, p. 755), James Danziger should have been identified as a political scientist.

BOOK REVIEWS

American Government and Politics

The Carter Years: The President and Policy Making. Edited by M. Glenn Abernathy, Dilys M. Hill, and Phil Williams. (New York: St. Martin's Press, 1984. Pp. viii + 227. \$22.50.)

Great expectations accompanied the start of the Carter presidency. Jimmy Carter had made many promises during the 1976 presidential campaign. Toward an economy burdened by stagflation, he promised noninflationary economic growth. Toward a people angry over various socioeconomic inequalities, he promised consumer protection, welfare, and tax reforms. Toward those Americans frustrated over governmental inefficiency, he promised to reorganize the bureaucracy and balance the budget. Toward a public growth cynical over Kissingerian Realpolitik, he promised a moralistic defense and foreign policy. Above all, he promised "a government as good as its people." Within four years these expectations had turned to dust. Defeated by the massive Reagan landslide, Carter left office with a reputation of incompetence and defeat. What happened? This question constitutes the focus of the 10 contributors to *The Carter Years: The President and Policy Making*.

The book's introduction suggests that initially Carter was placed at a disadvantage because of a resurgent Congress, a declining presidency, and a growing conservative mood within the nation, factors that would make the achievement of Carter's goals uncertain. These problems could only be overcome by a president who could impose a vision of what he wanted to accomplish, set priorities, and have a style, staff, and strategy that could work with Congress (p. 10). Subsequent essays indicate that, to one degree or another, ineffective communication of goals, disordered priorities, or inappropriate modes of legislative style added up to unrealized policies. For example, in his essay on domestic policy, Dilys Hill points out that Carter's tendency to overload the policy agenda, his failure to specify which proposals were of primary rather than secondary importance, and his aloofness toward the policy passage stage of legislation not only led to congressional rejection of most of Carter's proposals, but also to an image of amateurishness, a

deadly reputation in Washington. Hill also notes that Carter's proclivity to propose sweeping, comprehensive policy in a political system that tends toward incrementalism was inappropriate (p. 30). By the end of his term, Carter found Congress rejecting his proposals in such areas as health policy, tax reform, welfare reform, urban reform, campaign reform, and consumer protection. Although he was somewhat successful in the area of energy policy, much of that success was achieved amidst the politically damaging spectacle of long gas lines in the summer of 1979.

Raymond Moore's article on Carter's foreign policy notes that Carter wished to deemphasize East-West issues and in its place build a global community upon the moral pillars of human rights, disarmament, the resolution of regional conflict in Southern Africa and the Middle East, and more developmental aid for the Third World. However, only through gaining the Soviet Union's agreement to work for a common agenda—an agreement Carter never sought—could most of these goals be realized. Carter's failure to impose his idealistic vision on the Soviet Union and others pointed to the ultimate failure of his foreign policy.

Phil Williams's essay on defense argues that Carter's inability to get such key actors as the Armed Services, U.S. public opinion, and the Soviet Union to share his moralistic defense views led him to policy failure.

The Carter Years is a serviceable book; it presents its readers with a good description of what took place during the Carter years. What the book fails to do is explain adequately why events proceeded as they did. It fails to flesh out why Carter was unable to impose his vision on the domestic and foreign policy arenas, why he failed to order his priorities, or why he failed to develop a proper legislative-leadership style. The answers most of the contributors give to these questions seem rather facile. Is it enough to conclude that Carter's scattershot approach to legislation derived from his feeling that if he made many proposals, at least some would pass (p. 179)? Were Carter's problems with imposing his vision on domestic and foreign policy actors merely due to his inadequate communication skills? It could be

that the underlying cause of these problems was due to Carter's ambiguous feelings toward power. Or it could be that Carter's southern heritage influenced his presidential behavior. Clifton and Pierce McCleskey, authors of the book's best essay, suggest just that. They indicate that Carter developed a distaste for political parties from his experience as a Georgia politician, where the Democratic party was often incompetent and sometimes corrupt. Unfortunately, few of the other essays display this analytical power.

Interestingly, Carter's greatest achievement, the Camp David accords, which are hardly mentioned in the book, offers an example of where Carter's southern populist roots could be positively utilized and his (theoretically) ambiguous attitudes toward power could be resolved. For in his meetings with Sadat and Begin, who like himself were deeply religious, Carter could fully seek to accomplish the Christian goals of peace and goodwill and believe that power could be translated into God's missionary work on earth.

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The Supreme Court and Constitutional Democracy. By John Agresto. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 182. \$25.00, cloth; \$7.95, paper.)

John Agresto has written a book that tries to provide an outline for a new solution to the perennial problem of judicial review and democracy. "How," he asks, "do we, as a nation, give the Court the power and opportunity to guide us through our living and developing Constitution and still prevent the Court from substituting its principles of the Constitution, and elevating its rule over ours? In that question is the whole tangled problem this book has sought to unroll" (p. 160).

Agresto makes use of historical analysis of the origin and growth of judicial review to present a picture of an early judicial review deeply rooted in the principle of separation of powers. With Lincoln he deplores the emergence of the notion that judicial interpretation of the Constitution is final and binding on the other branches. Calling for a restoration of checks on the Court, he defends the effectiveness of Congress' power to ask the Court to reconsider its decisions (especially when supported by the force of public and scholarly opinion and timely use of the appointment

power). Yet, apparently sensing the inadequacy of such checks, Agresto calls for scholars to consider how they may be used more effectively.

The last chapter elaborates more fully the two goods Agresto wants to achieve at the same time. The Court is called upon to fulfill an important role in our society: not just checking legislation, but investigating questions of political and moral philosophy, fostering the growth of evolving national principles. At the same time, this must not be permitted to yield judicial oligarchy, especially with the developing power of the courts to give affirmative commands. "Checked activism" is the *via media* whereby these twin goals are to be achieved.

But "checked activism" is not the answer, in my opinion. The reasons why are hinted at in Agresto's own ambivalent attitude toward the Framers. He clearly has a high regard for them—no work is cited more often than *The Federalist*. But ultimately, he departs from their understanding of judicial power. His most frequent citation to describe the "activism" side of his "checked activism" is to Alexander Bickel, who expressly rejected *Marbury v. Madison* as too narrow a rationale. Like Bickel, but unlike the Founders I think, Agresto argues that the judiciary has been entrusted with the task of developing our evolving ideals and principles, seeing in them implications which the Framers did not have sufficient foresight to see. Perhaps it is not surprising, then, that Agresto implicitly criticizes the Framers for not attaching more effective checks to the judiciary (e.g., allowing an extraordinary congressional majority to override an exercise of judicial review). The Framers did not need such a check because they provided for a more limited judicial power, which would employ judicial review simply to enforce clear constitutional commands. I think that the Framers would have been rightly suspicious of whether "checked activism" would be sufficiently checked.

But my concerns about the solution proposed by this book should not obscure its considerable merit. *The Supreme Court and Constitutional Democracy* deals with a question which is the permanent question about national judicial power in America, and it makes a substantial contribution to our contemporary discussion by reminding us of the context of judicial review as it was originally conceived: separation of powers and checks and balances. Only relatively recently in American history has the notion of the finality of Court interpretations ("the Constitution is what the judges say it is") become so widespread, and Agresto's book shows why this view is such an unfortunate one. If it is not successful in its attempt to attain the best of both worlds (constitutional restraint and judicial activism), it does focus our

attention on the wisdom of the founding, which may point us in the right direction.

CHRISTOPHER WOLFE

Marquette University

Strong Democracy: Participatory Politics for a New Age. By Benjamin R. Barber (Berkeley: University of California Press, 1984. Pp. xvi + 320. \$16.95.)

Thomas Jefferson, Morris Janowitz (*The Reconstruction of Patriotism: Educating for Civic Consciousness*, University of Chicago Press, 1984), Harry Boyte (*The Backyard Revolution: Understanding the New Citizen Movement*, Temple University Press, 1981), and now Benjamin Barber have all made seminal contributions to the growing literature on citizen participation. Barber begins his closely reasoned philosophic work with Jefferson's often-quoted "no safe depository of the ultimate power of the society but the people themselves." The theme of this perceptive analysis is found in my own favorite quotation from Jefferson: "It is rare that the public sentiment decides immorally or unwisely, and the individual who differs from it ought to distrust and examine well his own opinion." Those who refrain from "deciding," for example, by not voting at elections, signal the bankruptcy of democracy according to Barber.

He argues for "strong democracy" as being far superior to what he terms three inferior types of democracy: anarchist, realist, or minimalist. Barber defines his preferred type of democracy as being politics in the participatory mode, resolving conflict through a participatory process of ongoing, proximate self-legislation, and the creation of a political community capable of transforming dependent private individuals into free citizens and partial and private interests into public goods (p. 151).

Barber aims "to associate democracy with a civic culture nearer to the themes of participation, citizenship, and political activity that are democracy's central virtues. We must do so without falling victim to either the nostalgia for ancient, small-scale republics that has made so many communitarian theories seem irrelevant to modern life, or to the taste for monolithic collectivism that can turn large-scale direct democracy into plebiscitary tyranny" (p. 25).

In Barber's view, the need for politics arises when some action of public consequence becomes necessary, and thus a public choice must be made that is reasonable in the face of conflict despite the absence of an independent ground of judgment. His response to the need for public action is

strong democracy, where conflict is resolved through the creation of a political community capable of transforming dependent, private individuals into free citizens and partial and private interests into public goods (p. 132).

The institutional reforms to achieve strong democracy include first, strong democratic talk (neighborhood assemblies, television town meetings and a civic communications cooperative, civic education and equal access to information, representative town meetings, and office-holding by lot); second, strong democratic decision making (a national initiative and referendum process, electronic balloting such as the Qube module in Columbus, Ohio, vouchers, and the market approach to public choice); and third, strong democratic action (universal citizen service and related training and employment opportunities, local options, democracy in the workplace, recreating the neighborhood as a physical public space).

Barber concludes: "This program does not illustrate strong democracy; it *is* strong democracy. . . . in the end, human freedom will be found not in caverns of private solitude but in the noisy assemblies where women and men meet daily as citizens and discover in each other's talk the consolation of a common humanity" (pp. 307, 311).

Are Barber's proposals for the revitalization of citizenship realistic and feasible? Can they be implemented in the twentieth century via the New England town meetings that I have attended and studied? Do the proposals require that all citizens be philosopher kings? Would the National Issues Forums of the Domestic Policy Association satisfy Barber's requirements? Will the so-called decline of our political parties mark the rise of "strong democratic talk and decisions and actions" at the neighborhood level throughout the length and breadth of our nation? Time will tell; the odds are against Barber.

NORMAN N. GILL

Marquette University

The Impossible Presidency: Illusions and Realities of Executive Power. By Harold M. Barger. (Glenview, Ill.: Scott, Foresman, 1984. Pp. ix + 450. \$9.95, paper.)

If the late 1960s and early 1970s epitomized the period of the "imperial presidency," the post-imperial presidency is characterized as the "impossible presidency" in this readable and interesting book. It offers a provocative challenge to Arthur M. Schlesinger, Jr.'s thesis in *The Imperial Presidency* (Houghton Mifflin, 1973). Indeed, it challenges the primary contribution of

post-World War II historians of the American presidency by questioning the validity of the presidential greatness game of Arthur M. Schlesinger, Sr. (*Presidential Greatness*, Irvington Publishers, 1966). In brief, Barger suggests that American historians have led the gullible public and the elite media down a primrose path via catch words such as "presidential power" and "presidential greatness" that has contributed to many of the problems of the post-Eisenhower presidency.

Barger seriously questions whether presidents have too much power. Going far beyond the White House mouse problem in the Carter administration, Barger cites abundant evidence of presidential weakness in the presidential roles of legislator-leader, administrator-leader, and free world leader. The decline of political parties prevents consistent leadership in the new decentralized Congress, the bureaucracy has grown too large for presidential management, and the changing global situation prevents America from running the world by itself. The myth of presidential power leads to many of the unattainable expectations of presidents and the public.

A major source behind the myth of presidential power is the notion of presidential greatness. Barger traces the quest for the perfect American president or the philosopher king to the post-Eisenhower phenomenon of the disposable president, because recent incumbents are unable to measure up to the greatness of Washington, Lincoln, and FDR. Barger particularly faults the media for accepting the notion that presidential greatness should be the norm. Americans should not expect all presidents to be heroes.

Barger rejects the traditional global reforms that consume so much time of many political scientists as being largely as unrealistic as myths of presidential power and greatness. He is more willing to settle for smaller innovations that might actually be accomplished and have some impact for the good. For example, rather than advocating establishment of a single six-year term or the abolition of the electoral college, Barger suggests that it might be more realistic and helpful (because past reforms often have caused many of our present problems) for scholars to hold seminars on the presidency for neophyte presidents and their staffs. One might extend this to the media and presidential candidates.

Historians are likely to challenge the thesis of this book with the latest and most sophisticated effort in the presidential rating game that suggests that our recent presidents have been underestimated when placed in historical perspective. For example, Robert K. Murray and Tim H. Blessing have found in their extensive polls of American historians that, except for Nixon, the nine presidents from 1929 to 1979 are now rated as average

or above: "No other fifty year period in American history, except for the era of the Founding Fathers and the early Republic, surpasses that performance record" (*Journal of American History*, 1983, 70(3), 552). In other words, the thesis of inadequate leadership in the modern presidency may itself be mythical. In any case, this clearly written, engaging, and economical paperback makes for a competitive possibility in American presidency courses.

WILLIAM D. PEDERSON

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The Interest Group Society. By Jeffrey M. Berry. (Boston: Little, Brown, 1984. Pp. x + 244. \$8.95, paper.)

Berry has undertaken a careful and thoughtful analysis of the role of interest groups in the American political process. His treatment of them is a valuable contribution to the literature of American politics.

The growth in the number and importance of pressure groups on the American political scene is familiar to all observers of our political life. Berry refers to it, accurately, as "the advocacy explosion." He discusses it within the context of "Madison's dilemma"—the age-old problem of "curing the mischiefs of faction." Early in his study Berry reviews the influence that pluralist thought has had within the discipline of political science. He notes that there is a growing, but certainly not new, popular perception that the power of interest groups is in some respects unhealthy.

The book gives particular stress to the reality that the waxing of interest groups has come during a period of party decline. The general weakening of partisanship and the rise in issue voting have been closely linked. Increasingly, more and more citizens look to interest groups to speak for them in the political process. The growth of interest group politics—whatever the attendant problems—has broadened representation within the political system. Yet the decline of the parties and the proliferation of interest groups clearly has worked to the advantage of those already well represented in the political process. Particular attention is given to the growing success and effectiveness of business pressure groups, notably the Business Roundtable. Berry also stresses that the appearance of new interest groups reflects changes in technology and the ever-increasing complexity of our society.

The analysis of the internal organization of in-

terest groups is well done. Here the "iron law of oligarchy" is pervasive. Many of these organizations are dominated by a small cadre of key workers. But the diversity in internal organizational arrangement is of considerable interest. A key problem for many organizations—a problem not faced by the single-issue pressure groups—is over the allocation of resources among issues.

Berry's discussion of lobbying is pragmatic and sophisticated, with some excellent analysis of the recruitment and background of lobbyists. There is a fine description of advocacy tactics. The growing influence of law firms, public relations firms, and the preeminence of superstars such as Tommy Boggs and Robert K. Gray are fascinating parts of the phenomenon of lobbying.

As would be expected, there is a major stress on the growing power and significance of political action committees (PACs). The substantial increase in the amounts of money being poured into congressional and senatorial campaigns is staggering. Even so, an enormous amount of money is spent by many PACs on administrative overhead, with a relatively small part of the money donated actually reaching candidates. The heavy support which most PACs give to incumbents is well documented.

Berry concludes that on balance the growth of interest group politics has furthered the democratic process. But it has not been without costs to our political system. He clearly embraces the Madison dictum that faction must not be fought at the expense of political liberty. The prospect for curbing the influence of factions—the "policy maximizers"—must come from a revitalization and strengthening of the political parties, the "vote maximizers." Berry rejects the notion that weak parties are an inevitable condition of American political life. He specifically calls for reform in the campaign finance laws so as to reduce the dependence of candidates on PAC funds.

A noteworthy feature of the book is the excellent bibliography. Berry's study will be of significance not only to students of interest groups and political parties, but also to all who are concerned about the political processes of American democracy.

RAYMOND H. GUSTESON

Ohio University

Redefining Human Life: Reproductive Technologies and Social Policy. By Robert H. Blank. (Boulder, Colo.: Westview Press, 1984. Pp. xvii + 270. \$22.95.)

Abortion and Woman's Choice: The State, Sexuality and Reproductive Freedom. By Rosalind Pollack Petchesky. (New York: Longman, 1984. Pp. xii + 404. \$22.95.)

These two volumes attempt to reflect upon the devastatingly difficult issues of human reproduction facing policymakers and indeed the entire political and social system, issues accorded special poignancy by the almost uncontrollable advance of technology in the late twentieth century. A glance at the titles of the books might lead one to assume that Petchesky's is the more narrow consideration, whereas Blank's is the heroic attempt to grasp the entire universe of problems. In the reading one is led to the opposite conclusion, for although *Redefining Human Life* by Blank is a prodigious effort to review both technology and policy response to it, it is *Abortion and Woman's Choice*, Petchesky's massive and yet elegant statement, that provides us with the bold, sweeping theoretical perspectives on human reproduction with which we can stage the policy debate in the largest arena. Blank does yeoman duty in presenting traditional liberal understanding of human life overlaid with traditional liberal attempts to manage technocratic invasion of those traditional assumptions, but insight into redefinition requires the most serious inquiry into the limits of liberal ideology when confronted with such questions, and Blank provides us with less help in making such inquiry than one could wish, given his ambitions. Petchesky, on the other hand, comes to grips with the problem immediately, setting forth a brilliant if controversial challenge to liberal ideology that gives the remainder of the book a coherence often reaching the point of devastating persuasiveness and, although one may (and doubtless many will) choose to argue with her initial assumptions, her own argument proceeds from them with a relentless internal logic.

Both Blank and Petchesky devote considerable effort to locating human reproduction within larger historical, social, and political contexts. Petchesky thoroughly challenges liberal assumptions about these contexts, especially with regard to recent attention to reproductive freedom as an extension of the right to privacy. Her perspective is clear from the outset: She will argue that "reproduction generally, and fertility control in particular, must be understood as a historically determined, socially organized activity" (p. ix) the consequences of which, for women and society,

are as obscured by liberal analysis as they are limited by liberal policymaking. Her perspective is both feminist and socialist, but one of her most brilliant accomplishments is to reach beyond facile acceptance of usual feminist or socialist "party lines" to critically examine both as rigorously as she critiques liberalism. Her early chapters, providing historical accounts of reproduction and fertility control from a social rather than biological or technological emphasis, employ this perspective so effectively that any reader should come away with a greatly enriched understanding of the accumulated weight of values and practices burdening contemporary society; the history she structures and articulates contributes a genuinely original voice.

Blank too strives to seat his argument in an interactive depiction of reproductive behavior and the political system. But his theoretical framework is unrelievedly pluralistic/incrementalist. From such a perspective Blank is unable to give adequate attention to one crux of reproductive control: How shall the state, attempting to "balance interests," finally resolve policy dilemmas when interests *can't* be balanced, when all attentive actors concede that the game is zero sum? Thus, much of the book consists of stating present policy dilemmas caused by technology, trying to anticipate future dilemmas (but once again from a pluralistic, liberal stance alone), and providing a careful but all too familiar evaluation of the system's likelihood to disaggregate and bargain away the policy questions. The near absence of reference to feminist formulations of these problems also contributes to the straits Blank's arguments are in, and for logical rather than ideological reasons. Both Petchesky and Blank cogently describe the system's varying justifications through time for reproductive policy in terms of population control, eugenics, pronatalism (for certain classes), and especially in terms of medical establishment models. But because Blank does not take the feminist perspective into account, only Petchesky can examine the complete array of consequences these justifications produce now and for future attempts to resolve the dilemmas. Blank's argument finds itself in the awkward position, for example, of arguing that the maternal right to control her own body is clearly established, but simultaneously relies on evidence that the courts are already moving in the direction of controlling maternal behavior in the interests of fetal rights to a safe environment; Petchesky's analysis removes the apparent confusion here with her convincing argument that the "woman's right to control" is not now and never has been firmly established. Blank asserts that the woman "alone makes the ultimate decision [regarding 'fetal environment'] . . . the fact is that it is the actions of

the mother, not the father, which affects [*sic*] the fetus" (p. 146), but at a later point attributes drastically reduced sperm counts in the American male population to environmental hazards; what he does not emphasize is that mounting evidence shows that sperm and sperm producing capability are qualitatively as well as quantitatively at risk from environmental hazard. This is only one way in which it is not the woman alone who makes these ultimate decisions, but women and men and social institutions who are implicated. Petchesky can account for such a formulation, but Blank cannot; what is even more serious is that the incrementalist policymaking system that Blank painstakingly describes cannot. It is to be regretted that Blank limited the number of alternative perspectives from which to construct his thesis, because the extraordinary compendium of technical and political information he offers us requires much greater theoretical cohesion than it now has if it is to be a guide to scholars and political actors.

Both books should be read, although opponents of abortion will find both books especially difficult. But the questions both books address are difficult no matter what one's personal feelings. *Redefining Human Life* is most useful as a clear description of current technology and system response. *Abortion and Woman's Choice* is a statement of political theory that should become a part of all interested scholars' repertoires as well as an invaluable aid to policymakers faced with these harrowing choices.

SUE TOLLESON RINEHART

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Civil Liberties and American Democracy. By John Brigham. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. xiii + 300. \$12.95, paper.)

This book is both a brief for and an exercise in "ideological jurisprudence," an approach that emphasizes the intellectual content of court decisions rather than the political or personal influences that may underlie them. Brigham acknowledges that politics plays a part in judicial decision making, but argues that political struggles that find their way to court are given shape and weight by the language and ideas of the Constitution and prior case law.

Brigham has produced an exhaustive summary of that case law organized under five comprehensive headings: freedom, due process, liberty, property, and equality. Noting that a chapter on property is something of an anomaly in a civil liberties treatise, Brigham contends that property

in American law has consisted of "settled or legitimate expectations" (p. 204) that deserve protection from state usurpation. "In this form," he concludes, "property is a fundamental right that we can again associate with civil liberties" (p. 205). One measure of Brigham's scholarship is that his index lists nearly 500 Supreme Court citations. Among other things, the book will serve as a useful reference work for the constitutional law student seeking a great volume of material reduced to manageable dimensions.

Several of Brigham's chapters are fairly straightforward syntheses of case law. Readers will appreciate Brigham's ability to connect and illuminate cases by focusing on ideas rather than outcomes. His discussion of due process in particular is a model of clarity and tight organization, all the more remarkable because it covers civil as well as criminal due process guarantees. Other chapters, however, have a fitful, start-and-stop quality about them, the result of Brigham's decision to give much more attention to some issues than to others. For example, in chapter 2, "Freedom," pornography is discussed in considerable detail, whereas freedom of religion is covered only in broad outline. Brigham explains that a more comprehensive discussion of the religion cases, with their often tortured lines of reasoning, would "focus on policy decisions and their consequences. It would not provide greater insight into the ideological structure of constitutional freedom" (p. 83). But the validity of Brigham's approach needs to be demonstrated rather than simply asserted; glossing over cases and issues that do not lend themselves to his framework tends to undercut Brigham's brief for ideology as a tool for analyzing constitutional law. Ideas are important in the law not merely because scholars say so, but because judges and lawyers treat them as important. When they do not, one expects the ideologically inclined author to offer some extended analysis of that failure.

Brigham's work bristles with challenging theses. Perhaps the most provocative is his argument that "the base for civil liberties is not the individual . . . but the citizen as person who was the source of a new kind of power" (p. 261). Because rights have meaning only in a social (or political) setting, they must be interpreted with reference to both their social causes and their consequences. Applying this standard, Brigham is critical of the doctrine of "pure tolerance," which he says fails to consider the social harm caused by pornography. He also deplores what he calls "pure equality," which celebrates "colorblindness" and "sex blindness" but ignores "the historical situation that produced the constitutional standard in the first place" (p. 246). While faulting the "color-blind" Supreme Court justices who handed Allan

Bakke his victory, Brigham also takes Equal Rights Amendment advocates to task for adopting language that promotes the goal of "pure equality" over that of protecting women against discrimination.

Brigham's critique of civil liberties law does credit to the intellectual tradition in which he works. One does wish that he had not chosen to speak in more than one voice. His first and last chapters, written in a textbook-like tone, are padded with material that any reader sophisticated enough to appreciate the insights of the middle chapters will find irrelevant. On the whole, however, this is "ideological jurisprudence" at its best: by taking legal ideas seriously, Brigham compels us to take *his* ideas seriously.

RICHARD J. MAIMAN

University of Southern Maine

Bureaucratic Power in Society. by Richard Chackerian and Gilbert Abcarian. (Chicago: Nelson-Hall, 1984. Pp. 208. \$23.95, cloth; \$11.95, paper.)

In this relatively small volume the authors construct a Maslow-Weber labyrinth and attempt to lead the reader to the exit. Unfortunately, even if one is well versed in a wide range of sociological concepts, the exit is at best elusive.

The volume starts with the assertion that "Today, there is a crisis of trust in governmental and corporate institutions" (p. 1). It ends with the conclusion that "Unfortunately, the decline in the legitimacy of bureaucratic power has been associated with a weakening ability of industrial bureaucracies to meet material needs, with family arrangements that are increasingly unable to provide adequate emotional support to children, and with a political system that is less controllable by its citizens and even less capable of supporting material needs" (p. 175). In between, Chackerian and Abcarian attempt to demonstrate how personal development (Abraham Maslow's self-actualization) has been inhibited by the diffusion of bureaucratic values (Max Weber) to primary institutions (family, school, and polity).

Chapter 1 states the issues to be explored: "The diffusion of bureaucratic values to primary institutions and the individual consequences of this diffusion. . ." (p. 12). The following chapters develop the major tenets of the work, for example, in chapter 2, the nature of bureaucracy (Weber) and human needs (Maslow); in chapters 3 and 4, the diffusion of bureaucratic values. The remainder of the work ostensibly develops ideal types for the family, polity, and school; examines the extent to which these primary institutions

meet the models; and concludes that there exists a crisis of trust and legitimacy resulting from the diffusion of bureaucratic (instrumental) values to primary institutions thereby voiding or weakening intrinsic values.

Although the objective and framework of this volume make sense intuitively, the substance of the work is presented confusingly, some is not relevant to the thesis, and the overall conclusions of the work are unsupported.

Chapter 3 is a review of the socialization process as it relates to diffusion of bureaucratic values to the family, school, and the polity. The chapter, "Socialization as Diffusion," is illustrative of the ambiguity of concepts, or the ambiguity of relationships among various concepts, evident throughout this work. The volume appears to be a compilation of concepts that are unassimilated into an understandable bureaucratic values diffusion and personal development model.

To illustrate, chapter 7 is one of three written to provide "Specification of ideal models [which] will provide the criteria for evaluating the actual forms of these institutions [family, school, and polity] . . ." (p. 70). Yet no ideal models are presented or evaluations made. Rather, chapter 7 sets out to "examine several aspects of [the] crisis in schooling. . ." (p. 111). The authors conclude by stating that "A 'hidden curriculum' teaches children to accept certain social myths as reality, in particular the virtues of product consumption and gratification of material wants" (p. 124). And, "educational research identifies bureaucracy and the diffusion of bureaucratic values as a major factor in the contemporary educational crisis" (p. 125).

These conclusions typify the problems with the work. At best, they are over simplifications; at worst, they ignore so many other relevant factors as to be incorrect. It seems to me that one can hardly ignore the impacts of the "individual rights" movements that dominated the U.S. political and social scene for most of the 1960s and 1970s, most of which ran directly counter to the imposition of standardized, bureaucratic values. One could argue that, at least for this time period, the emphasis on individual rights as made manifest in school integration, family values, women's roles, and voting rights had a greater impact on primary institutions than the diffusion of bureaucratic values. Perhaps one should ask females and blacks whether their personal development potential is greater or less now than before 1960.

The work concludes with the chapter "Loss of Trust and Legitimacy." The point made is that the "legitimization of the ideas of hierarchy and impersonality . . . have detracted from the ability of the family, school, and polity to perform . . .

their core functions of meeting individual needs for affection and personal growth" (p. 154). The net result for these primary institutions is a weakened "efficacy in generating trust and legitimacy" (p. 154).

Are the core functions of family, school, and polity to provide affection and personal growth? How do these relate to trust and legitimacy in governmental and corporate institutions? How does any of this relate to Maslow's highest level of human needs, self-actualization?

There is general acceptance of the notion that individuals' higher level needs (self-actualization in Maslow's terms) are difficult to satisfy in large, impersonal bureaucratic institutions. However, the conclusion that the instrumental relationships that characterize bureaucracies have become so diffused in our primary institutions that personal development is thwarted, and that this has resulted in a crisis of lack of trust and legitimacy in government, is not made convincingly.

GERALD W. JOHNSON

Auburn University

City Money: Political Processes, Fiscal Strain and Retrenchment. By Terry Nichols Clark and Lorna Crowley Ferguson. (New York: Columbia University Press, 1983. Pp. ix + 440. \$37.50, cloth; \$15.00, paper.)

From the mid-1970s, there has been rapid development of a substantial literature in the field of urban fiscal strain. Common to most of this literature has been the attempt to devise measures of fiscal strain and to offer plausible causes for, and feasible solutions to, this phenomenon. *City Money* can be viewed initially as a cut above the rest of the works in this area, because it represents a commendable effort to develop the following: (1) improved measures of fiscal strain; (2) a comprehensive model to explain strain, which was produced by diligently pulling together a disparate literature; (3) reasonable courses of action that cities may take to cope with this problem; and (4) indepth analyses of several cities.

Although not directly stated, it is implicit from a reading of this book that past studies have failed to capture the full scope of fiscal strain. Clark and Ferguson define fiscal strain as "poor adaptation of (1) fiscal policy outputs of the city government to (2) private sector activities. . ." (p. 45) and measure it with a ratio dividing (1) by (2). The various ratio indicators use four different numerators (general expenditures, own revenues, common functions, and debt) and five denominators (median family income, population change from 1960 to 1970, population change from 1970 to

1974, population change from 1974 to 1977, and city wealth index). The various combinations of numerators and denominators produce 20 separate indicators of strain used as different measures of the dependent variable for purposes of testing the model.

According to Clark and Ferguson, the construction of a model that increases our knowledge of the sources or causes of strain necessitates dealing with political decision making (p. viii). After noting that "past work on fiscal policy has often dismissed politics as idiosyncratic and hence amenable to anecdotal treatment" (p. viii), they offer a systems analysis model of urban fiscal strain that posits specific linkages between the economic base, political decision making, and urban fiscal strain (chap. 1). Much of the remainder of the book (chaps. 4-8) is concerned with presenting a sound theoretical justification for inclusion of the independent variables in the model (i.e., citizen preferences, organized group activity, political leader attitudes, white ethnic and black power influence, municipal employee unions, tax preferences of the middle class, and migration) and testing the significance of each with the aid of data from the Permanent Community Sample (PCS) of 62 cities. Whereas this portion of the book would probably be of greatest interest to those concerned with theory-building, chapter 10 could be most appreciated by the practitioner, because it takes the knowledge obtained from the testing of the model and translates it into viable strategies aimed at reducing fiscal strain.

Nevertheless, several troublesome aspects of the book potentially compromise its quality and usefulness. These areas of concern are as follows: (1) frequent use of an unorthodox significance level for testing hypotheses (i.e., $\alpha \leq .10$); (2) questionable shifts in the level of analysis (e.g., from city to county in migration analysis in chapter 8); and (3) incidences of making generalizations about influences of statistically insignificant variables (e.g., "1974-77 changes in employee numbers," pp. 164-165). Furthermore, the book often suffers from poorly constructed tables and figures and awkward sentence structure that serve to confuse and bewilder the reader. Finally, the nature of the PCS makes it difficult (if not impossible) to report findings on the basis of region, population size, and central city or suburban status. Yet each reader will have to make a judgment as to how serious these problems are and to what extent they undercut the utility of the book.

J. EDWIN BENTON

University of South Florida

Studying the Presidency. Edited by George C. Edwards III and Stephen J. Wayne. (Knoxville: University of Tennessee Press, 1983. Pp. viii + 312. \$9.95, paper.)

For the media and the public, the president is, if not the personification of government, at least its major component. By contrast, political scientists have devoted far more attention to the study of Congress. In his introduction, Stephen J. Wayne argues that this is due both to the institution of the presidency and the difficulty of obtaining the kind of data that lends itself to either quantitative analysis or reliable generalization. But he believes that scholars are beginning to overcome these problems and hopes that *Studying the Presidency* will "stimulate them further" (p. 10).

The book is divided into two parts, the first discussing different approaches taken by students of the presidency and the second providing a guide to research sources and techniques. Although the essays vary in quality, they are better integrated in goals and approaches than is usual in such collections. "Approaches," by Wayne, begins with a clear and sensible survey of the literature, dividing it into legal, institutional, political, and psychological perspectives. Although a good introduction for the beginning student, it will serve as no more than a review for those reasonably familiar with the field. More interesting are the remaining three selections. Building on work by Harry Eckstein, Norman C. Thomas demonstrates that case studies have suggested and tested important theoretical propositions. George C. Edwards III makes the case for quantitative analysis. Because so much has been written on this subject, his failure to break new ground is hardly surprising, but he does provide some useful cautions about the pitfalls involved in using aggregate data and what quantitative analysis cannot do. John Kessel's account of his own research serves to remind us that the polished products we read in books and journals often come from a series of gropings that resemble educated guesses more than confident theory-building.

The second part of the book concentrates on gathering data. It consists of three essays about data sources available in research libraries, two about presidential libraries, and two about interviewing presidential aides. Jennifer De Toro provides a list of available sources of information about the presidency, with a brief description of what each does and does not include, but she does not analyze the strengths and weaknesses of each or the problems they can pose for researchers.

A more critical review of sources on executive-legislative relations by G. Calvin MacKezie does better, but is hampered occasionally by sketchiness, as in his discussion of the *Congressional*

Record. MacKenzie suggests that one of the publication's major purposes is "to provide a verbatim transcript of everything said on the floor of both houses," but it is not until several paragraphs later that we learn that we should "be careful to try to distinguish that which was actually spoken on the floor from that which was merely inserted for publication. The *Record* includes some helpful keys for making that distinction, but none of these is fully reliable" (pp. 162-163). Not only should MacKenzie have told the reader what these keys and their limitations are, he should also have pointed out that members are permitted to "revise and extend" their remarks, which makes the *Record* less than reliable as a verbatim transcript. Louis Fisher's essay on legal sources could also use more length, because these are the sources most likely to be unfamiliar to political scientists. Fisher begins by defending the importance of the legal perspective. Because this part of his essay is too brief to convince any skeptics, it should either have been expanded into a full article for the first part of the book or omitted entirely, allowing his list and brief description of legal sources to have been expanded to include more critical discussion as well as harder to locate sources such as briefs and oral arguments.

Martha Joynt Kumar does a fine job of evaluating the utility and limitations of the material available in presidential libraries for understanding the decision-making process and developing and testing hypotheses. By contrast, Larry Berman mixes a few useful practical suggestions with long extracts from library materials, most of which can be obtained by writing to presidential libraries.

The two essays on interviewing provide the kind of contrast one expects between journalists and scholars. The journalist, Dom Bonafede, provides many useful do's and don'ts without exploring them too systematically or in too much depth. The political scientist, Joseph Pika, has written the best of the practical chapters in the book. In a nontechnical way, he explains how to gain access, prepare for the interview, decide what to ask and how to ask it, establish rapport, deal with unexpected problems, and analyze the resulting data.

The editors are to be commended for addressing an important area that needs attention. Unfortunately, the effort falls far short of the need. The articles by Thomas and Pika should be of wide interest, that of Kumar of interest to those who are considering utilizing presidential libraries, and the others primarily of interest to those little acquainted with research on the presidency.

BRUCE E. ALTSCHULER

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Public Sector Management, Systems and Ethics.

By Louise C. Gawthrop. (Bloomington: Indiana University Press, 1984. Pp. 173. \$22.50.)

There is no dearth of critical analyses of contemporary organizations in the United States. We know that bureaucracy has become dysfunctional, particularly in many government settings. Clearly, the classical bureaucratic organization continues to offer certain advantages such as the application of consistency, continuity, and predictability to the organizational environment through routinized decision-making and problem-solving procedures. The dilemma that must be confronted today, however, is that the organizational environment has grown exceedingly complex and well beyond the capacity of classical organizations to deal effectively with rapid social, economic, technological, and political change. The critical task today is to redesign our obsolete organizations to cope with the complexity of change.

Louis C. Gawthrop's critique of classical public organizations (his "network X"), although adequate, adds little to our knowledge or awareness. Rather, the real contribution of this brief but thoughtful and thought-provoking volume is in bringing together the notions of management, systems, and ethics into an integrated metasystem for analyzing and substantially redesigning our public organizations. Gawthrop thus moves beyond the conventional nail-biting and teeth-gnashing literature on the problems and shortcomings of modern organizations into new theoretical territory, advocating a novel way of thinking about organizations and demonstrating how redesign can make them more capable of continuously adapting to the rigorous demands of the future.

Drawing eclectically from the theoretical work of Ludwig von Bertalanffy and others, Gawthrop prefaces his argument with a discussion of general systems theory and its essential components of holism, teleology, hierarchy, and temporality. Applied to these four systems components are the "essential ontological concepts of sense of purpose, sense of consequence, sense of history, and sense of order" (p. 38). The result is a model of a new public sector organization, referred to by Gawthrop as "network Y," that promises to achieve "a horizontally focused, 'dynamic pathway' through society to a purposeful reality; a capacity to increase the level of anticipatory knowledge of the future; a capacity to monitor the changing causal texture of the social order on a day-to-day basis . . . and a capacity to enable public sector decision makers to undo the negatives of past policy, programmatic, and operating decisions" (p. 38). The principal objective is to redesign organizations so that public administra-

tors can strategically adapt to frequent, complex demands for change.

Recognizing that the bulk of government policy activity will continue to be composed of routinized, highly programmed performance activities (the organization's "maintenance system"), Gawthrop's proposed new organizational design integrates traditional program maintenance functions with future-oriented systems management and planning. The result is an organic, matrix-type organization that operates with strong information processing capabilities and little hierarchical differentiation, except at the level of the chief executive officer who maintains political and policy responsibility for the organization. Importantly, the network Y organization moves beyond the ateleological nature of systems design to "a transcendent sense of purpose" that encourages the capacities to find objective reality, to accept tension as value-enhancing, and to view "criticism and criticalness as positive components of qualitative growth" (p. 127). For public administrators, effective management becomes a product of individual responsibility for actions and a "critical consciousness" set within "a creative ethical framework that is conducive to a systems notion of organizational responsiveness and responsibility . . . [and] congruous to the basic tenets of democratic theory" (p. 137).

Gawthrop is disturbingly short on examples of network Y in practice. Nonetheless, while Gawthrop's "ethics of civility" may ultimately prove to be a pipe dream and network Y unattainable, *Public Sector Management, Systems, and Ethics* constitutes a major contribution to the literature of public administration generally and public organization theory specifically. It unquestionably elevates the level of debate on the shape of organizations of the future and is certain to leave its mark on the literature and, one would hope, on the practice of public administration.

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Campaigning for Congress. By Edie N. Goldenberg and Michael W. Traugott. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. 207. \$9.50, paper.)

Congressional election campaigns have been examined in only limited ways in congressional research. Analyses have dealt with losers more often than winners, studied contests in one state, and portrayed a single candidate's campaign. In *Campaigning for Congress*, Goldenberg and

Traugott move beyond these specialized accounts to provide a more general, thoroughgoing report on congressional campaigns. The book is both conceptually and empirically important. Conceptually, it presents campaigns as dynamic, interactive occurrences of the candidates together, rather than discrete enterprises of each candidate individually. Campaigns unfold over time, as the strategies of one candidate are inseparably linked to strategies of the other candidate. Additionally, the authors stress the degree of electoral uncertainty as a key element of the campaign. The interrelated strategies of how to run are ultimately defined by the uncertainty (or certainty) of who will win. Empirically, these themes are developed by a close examination of 86 House races in 1978. Data were gathered in each district from interviews of the campaign managers for both major party candidates, expenditure records measured at several points throughout the full pre-election period (1977 to 1978), media market information, and voters' survey responses.

Relying on the interview data, the authors characterize how campaign managers continually evaluate the electoral uncertainty of their races depending upon relative party strength and the shifting competitiveness of the candidates. From the managers' assessments, a typology is proposed depicting sure winners and vulnerable incumbents, sure losers and hopeful challengers. Notable differences exist in the ways these four types of candidates approach their campaigns. Although the discussion of managers' strategies and tactics is at times overly descriptive, it provides an inside view of how campaigners perceive the campaign.

The most intriguing part of the book focuses on the dynamics of campaign spending and candidate uncertainty. Although most research has measured the total aggregated impact of campaign spending on congressional races, Goldenberg and Traugott monitor quarterly candidate expenditures for the entire interelection period. They observe preemptive spending by incumbents who finance campaign activities well before their challengers begin their campaigns. Incumbents thereby minimize uncertainty early and foreclose later challenger efforts. In addition, evidence of reactive spending is uncovered as candidates spend competitively in races perceived to be close.

The book also breaks new ground in analyzing media influence on the campaign. Regardless of uncertainty, all candidates spend greatest amounts of money on media expenses. The authors calculate the efficiency of television and newspaper markets. The more closely the district and the market overlap, the more likely the campaign will be covered, and the more valuable money spent on media exposure will be.

Finally, the book considers voters' responses to the campaign. A model linking the campaign to voter turnout, information, and choice is suggested. Unfortunately, the direct link posed in the model is explored only by inference. Basic knowledge of voters is presented without considering key elements of the campaign as revealed in the campaign manager interviews. Integrating managers' perceptions of uncertainty and of the campaign with voters' attitudes and actions would have enriched the study.

Nevertheless, the book provides the most comprehensive treatment to date of congressional campaigns. The breadth of the data and the development of the themes of uncertainty and strategic actions and reactions of candidates make a worthwhile addition to understanding congressional elections.

LYN RAGSDALE

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TVA: Fifty Years of Grass-Roots Bureaucracy.

Edited by Erwin C. Hargrove and Paul K. Conkin. (Champaign: University of Illinois Press, 1984. Pp. ix + 345. \$24.95.)

This work provides social scientists with a rare opportunity to consider a large governmental agency over a long time frame. We are in a position to examine Philip Selznick's classic theory of cooptation (*TVA and the Grass Roots*, Harper & Row, 1966), Marver Bernstein's life cycle of the bureaucracy (*Regulating Business by Independent Commissions*, Greenwood Press, 1955), as well as other theoretical studies in the light of history. Although there are references throughout to various theoretical works, the various essays in this book focus more on the idiosyncracies of the TVA. With the exception of the theories noted above, the reader is required to put the TVA in a broader theoretical context. As Hargrove notes in his introduction, "We have written analytical history rather than theory" (p. xvi).

The 50 years of the TVA highlights some important themes. "Grass-roots democracy" was more talk than substance. The apolitical bureaucracy is a myth. Expertise is an important political resource for the bureaucracy. Bureaucracies need not ossify.

Although I fault the work for not developing the theoretical implications of organizational leadership, organizational change, bureaucratic power, and the like, this book is a wonderful resource. It is composed of 12 essays, each of which is meticulously researched, well written,

and full of insights on the changing role and changing light in which the TVA is viewed.

The work holds together very well. It is organized into four sections: "The Formative Years," "Leadership and Government," "Contemporary Problems," and "TVA and American Democracy." The first part presents the historical evolution of the TVA. Paul K. Conkin explains the legislative history of the TVA Act. The novelty of the idea allowed great bureaucratic discretion, thus great opportunity and great uncertainty. Richard Lowitt traces the early development through the glory years of World War II. He notes early conflicts and the tremendous public relations value of grass-roots democracy. It is clear that in reality those benefited were the rich farmers and not the grass roots. Wilmon H. Droze explains the TVA since 1945. This period is marked by intermittent warfare with Congress and a coalition of private power advocates. More recently, its close relationship with the AEC and its headlong thrust into nuclear energy has tarnished the TVA's image.

Part 2 includes an article by Hargrove which profiles the leadership styles of the seven TVA chairmen. He concludes that Bernstein's theory is not applicable, that leadership requires bargaining, management, and agenda-setting skills, and that, "The 'myth' of the apolitical organization has been a political resource sustained by politics" (p. 117). Avery Leiserson argues that political insulation provided by the TVA's organizational status has allowed it to be flexible. Although that insulation from politics gives the TVA a wide berth, a keen awareness of political reality has made the TVA successful and generally accountable. Vernon W. Ruttan believes that the TVA has been successful because it has had the human resource capability to adapt to a changing economic, technological, and political environment.

The third section highlights contemporary problems. The essays highlight some common themes that can be summarized as an arrogance of power. William Bruce Wheeler and Michael J. McDonald note an ambivalence about public involvement and a technocratic outlook. Dean Hill Rivkin states that public interest law forced TVA to consider social costs as part of their technocratic benefit-cost ratios. Richard A. Couto notes that TVA was a victim of its own rhetoric when low-cost power gave way to higher costs.

The last section deals with the democratic implications of the TVA. Cranford D. Goodwin deals with attempts to expand the Valley Authority concept and concludes that political timing and problems of coalition formation led to failure. William C. Havard notes the clash of technocratic values with humanistic values. The

early successes of the TVA represented a clear victory for the former. In the long run, however, the transformation of the South from rural to urban is not without its costs. Dewey Grantham also notes the ambiguous status of TVA. Certainly, it has been quite successful with regional development but has contributed less to social reform.

In sum, this is an interesting data base with many keen insights for students of legislative process, the bureaucracy, and public policy formation.

ROBERT W. KWEIT

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Presidents and Their Parties: Leadership or Neglect? Edited by Robert Harmel. (New York: Praeger Publishers, 1984. Pp. ix + 275. \$26.95.)

This edited volume makes a worthy contribution to the study of the American presidency by focusing on the president's role as party leader. In the past, the relationship between presidents and parties has been treated with perhaps a chapter in some textbooks on the presidency; this is the first sustained treatment of the subject in book form that I know of. This book attempts to survey the complex story of president-party relations, which according to Harmel are based on "an interplay of tradition, events and necessities of the moment, presidents' backgrounds and personal political skills, and the governmental, political and social context within which presidents and their parties coexist" (pp. 15-16). In order to do this survey, Harmel includes essays by Ralph Goldman, Harold Bass, Howard Reiter, Roger Brown, Sidney Milkis, George Edwards, III, Jeff Fishel, and two by himself.

Goldman argues in "The American President as Party Leader: A Synoptic History" that presidents fit into four factional types: the non-partisan, the subpartisan (a factional leader within the party), transpartisan (moving across party boundaries), and the partisan. Moreover, he details five cycles of presidential partisanship and names the cycles after the respective leader of the cycle: Madison, Van Buren, Chandler, the Barnum-Hanna cycle, and the Farley cycle. Goldman suggests that Reagan could be the new cyclical leader for the 1980s.

Harold Bass looks into relations between the White House and the national party organization and finds similar themes of White House dominance, White House selection of party staffers, and White House supervision of national party activities. Howard Reiter examines the resources presidents can use in trying to influence the out-

comes of national party conventions, and Roger Brown finds that "presidents as campaigners for their parties have been motivated by self-interest rather than altruistic urges to support and strengthen the party system" (p. 146). Sidney Milkis concentrates on Franklin D. Roosevelt's unsuccessful efforts to purge his party in 1938.

In two other important chapters, Edwards and Fishel deal with the president and the party in government. Edwards, in "Presidential Party Leadership in Congress," points out the severe limitations that American presidents have in their efforts to act as party leader for the U.S. Congress. Although the president may try to work with party leaders of the House and the Senate, and although he may hope that that shared policy preferences will guide congressional behavior, the president is often frustrated in his efforts to lead because of congressional decision making that is influenced by narrow district concerns, constituent service, and the pressures of reelection.

Fishel, in "Presidents, Parties, and Platforms: From Campaign Promise to Presidential Performance," makes significant points that platforms and candidate issue papers provide useful information about the broad outlines of expected presidential policy. Even though these documents may be unclear, they can provide directions in keeping elected officials accountable to some standards as measured by their fulfillment of promises. Of course Fishel is sophisticated enough to note the problems with this kind of study, such as promises that are ambiguous or that could not possibly be kept, yet he has built upon the works of other scholars in his attempt to use quantitative content analysis of presidential campaign speeches, position papers, and platform representations.

Harmel has done an excellent job of editing a volume to fill the void in the study of presidents and parties. Like any edited volume, the quality of the articles varies, but this volume is consistently on the mark in focusing the study of presidential-party relationships. As Harmel observes, "Whether they like it or not, party and presidency have become too interrelated over the years to allow either to totally dismiss the other. The future of the parties—and, to a lesser extent, the future effectiveness of the presidency—will be affected by the nature of their relationship" (p. 262). This volume goes a long way in helping us to understand those relationships.

JOHN ORMAN

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Strategies for Effective Desegregation: Lessons from Research. By Willis D. Hawley et al. (Lexington, Mass.: D.C. Heath, 1983. Pp. x + 210. \$23.95, cloth; \$13.95, paper.)

A joint effort by nine coauthors representing several disciplines, this book blends advocacy and analysis in a generally systematic and often compelling fashion. Each chapter employs a format of topical discussion followed by evidence from the literature and, when available, examples from desegregation experience. Hawley and associates offer readers (1) reviews of the school desegregation literature; (2) proposals to reduce racial isolation in public schools and to avoid resegregation in schools already desegregated; (3) recommendations for building student, parental, and community support for desegregation; (4) suggestions to change curriculum, student evaluation, instructional methods, and teacher training for the sake of "more effective" desegregation; and (5) an attack on the "new mythology" about school desegregation, said by the authors to combine pessimism and misinformation.

Academics will find the book most useful for discussion of the impact of different types of desegregation plans on white pupil loss, and a brief but very good treatment of reciprocal relationships between segregated schools and segregated housing.

A more general audience, including parents, school administrators, and others directly involved in school desegregation, will profit from chapter 7's discussion of how smaller schools can promote more teacher attention to individual pupils, perhaps improve educational quality, and otherwise help ease racial tensions during the first year or so of implementation. Other chapters offer useful suggestions for parental involvement in school activities, maintaining contact with families whose children have left the public schools, and publicizing positive aspects of desegregation.

To their credit, Hawley et al. declare that their proposals should not be regarded as "foolproof recipes" or suitable for all locales. Readers are invited to draw their own conclusions about the feasibility of each recommendation.

I suspect readers will heed this good advice when they come upon some of chapter 7's ideas to transform public schooling in the cause of interracial harmony and minority student self-esteem. Here desegregation is taken as a license to "innovate," and the "multi-ethnic" approach advocated appears bent on avoiding almost any scholastic situation in which pupils figure as individuals rather than members of categorical groups. Whether educational quality will be enhanced as a result is open to serious question, and widespread

doubts on this score surely contribute to the pessimism lamented by the authors.

The book would have benefited from more systematic discussion of the role of middle-class white pupils in school desegregation. Chapter 3 advocates reassignment of as many such students as possible to serve as achievement role models for minority and lower-income white classmates, but chapter 7's approach appears to minimize the white achievement thesis by accentuating racial differences and favoring "pluralistic" standards. Still another topic needing more development is minority diversity. Occasional references to limited-English proficiency and foreign-born students do little to disaggregate so variegated a grouping. Too often "minority" seems to mean "lower-income black."

Despite these reservations, *Strategies for Effective Desegregation* should be carefully read by academics, educational administrators, and members of the general public interested in one of the nation's most difficult and enduring controversies.

EMMETT H. BUELL, JR.

Denison University

The Frustration of Policy: Responses to Crime by American Cities. By Herbert Jacob. (Boston: Little, Brown, 1984. Pp. xiv + 188. \$9.95, paper.)

The Politics of Law and Order: Street Crime and Public Policy. By Stuart A. Scheingold. (New York: Longman, 1984. Pp. xiv + 238. \$25.00, cloth; \$13.50, paper.)

Street crime is a political issue in which symbolic gestures and the appearance of action often become substitutes for substantive policies. The two books reviewed here drive home this point in different but complementary ways.

Jacob's book is based on a massive investigation of the political histories of 10 cities and their criminal justice systems as they grappled with the rise in crime between 1948 and 1978. Douglas Yate's "ungovernable city" thesis (*The Ungovernable City*, MIT Press, 1977) provides Jacob with a basic perspective of the evidence indicating that although the cities expanded their law enforcement activities, "those expansions were not simply a response to crime" (p. 166), and for his conclusion that the governments did "many little things while pretending to pursue a grand strategy" and that often "they believed their pretensions" (p. 165). The political

saliency of crime rose throughout the 30-year period in all 10 cities, but at the same time local policy agendas grew more crowded, elections became more heated and closely fought, and divisive issues such as race relations arose which when combined reduced the chances for effective anticrime programs. These difficulties were compounded further by the fragmentation of local governments, the absence of cohesive policy-making bodies in the law enforcement area (which reflected the proliferation of local political power bases according to a rather controversial measure used by Jacob), the unrecognized fact that national forces shaped the dynamics of local crime patterns, and last but not least the lack of reliable technological knowledge to design programs. In the face of these obstacles and the tenacity of crime, it is little wonder city officials maintained the illusion they were responding to the problem.

Scheingold's perspective rests on his claim that "conflict criminology," which views crime as the "politization of social conflict" (p. 28), offers a better vantage point to explore crime and its political impacts than the "mainstream" views of such scholars as James W. Wilson or the Marxist alternative of criminologists like Richard Quinney. After challenging the mainstream's assumptions about crime and reform as well as Marxist expectations regarding legal repression, Scheingold states the politics of law and order often is "just so much sound and fury" (p. 204). Its effects are problematic and less intense than these perspectives suggest because its impulses "tend to be refracted by the political and bureaucratic forces that control the course of criminal process" (p. 28). Scheingold's book, then, is an extended argument with mainstream and Marxist writers over the cultural antecedents and dynamics of popular fears about crime and attitudes about criminal punishment, the role of the news media and entertainment industry in perpetuating and shaping these views, and how the cultural politics of law and order reinforce the punitive views of the police on the street and reduce the chances for reform while clashing with the less punitively inclined values of the criminal courts.

Both books are strongest when discussing crime as a political issue and when analyzing the police response. Scheingold draws on existing research to show that public fears of crime and attitudes about punishment are at best weakly related to crime rates or personal victimization. Pointing at the newspapers, television, movies, and popular novels, he suggests the public learns about crime and how to respond to it from these sources rather than from experience. His chapters on the police summarize the literature on their occupational values and behavior on the streets and raise questions about the feasibility of alternative policing

styles under current conditions. Jacob's empirical data buttress much of Scheingold's argument. His analysis indicates that newspapers increasingly emphasized violent crimes while expanding their coverage of crime over the years. More importantly, the prominence of crime on urban policy agendas failed to reflect actual crime rates and patterns. The politics of crime, then, seems to have taken on a life of its own. This conclusion gains further strength from Jacob's fascinating look at the response of police departments. Whether the measure was changes in police expenditures, arrest rates and priorities, reorganization of departments, or appointments of new police chiefs, the driving force rarely was crime rates according to Jacob's analysis.

The weakest parts of the books are their sections on the criminal courts and their conclusions. Jacob can say little about sentencing trends because the necessary data were not available. He confines his analysis to changes in court resources and how appellate and local court decisions shaped local politics. There is an important chapter on changes in local ordinances and state criminal laws, particularly the recent move toward determinate sentencing, but the impact of these changes on local sentencing practices could not be documented easily. Scheingold defends the guilty plea process using a "bureaucratic justice" model and argues that "due process liberals" are co-opted by law and order advocates on the issue of limiting the discretion of court officials because they lack a normative theory of punishment. The force of his argument is weakened, however, by his misreading of the policy-oriented research in this area, which is chiefly concerned with determining how much punishment is needed to achieve different policy goals and the costs of these measures than with disparities and discretion *per se*. Finally, the conclusions of the two books are disappointing. Jacob's recommendation that sentences be made shorter in particular simply has no foundation in his book. Scheingold's argument for "neighborhood justice" similarly lacks much grounding in the discussion that precedes it, and it is unclear as to how the idea would operate given the cultural "constants" he takes pains to identify.

Despite their flaws, these books ought to find receptive markets. Jacob's book is concise and crisply written, the analysis is presented in a straightforward manner, and it addresses challenging political and policy questions. Scheingold's volume is less tightly organized in part because it is more ruminative, but also because the conflict criminology perspective gets blurred as the argument proceeds. Nonetheless, it stands as a useful alternative to the more orthodox mainstream and Marxist literature on the politics of

crime. Both books warrant the thoughtful attention of their readers.

ROY B. FLEMMING

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Firearms and Violence: Issues of Public Policy.

Edited by Don B. Kates, Jr. (San Francisco: Institute for Public Policy Research, 1984. Pp. xxxiii + 571. \$38.00.)

The evidence of this volume is that there is no direct link in the contemporary United States between changes in levels of gun ownership and interpersonal violence, or between gun control and reduction of violent crime. On the first of these points, careful quantitative analysis by Gary Kleck suggests that conventional wisdom has the causal connection wrong: in fact, increases in violent crime tend to lead to increased gun ownership, presumably for defensive purposes. On the second point, analysis by Joseph P. Magaddino and Marshall H. Medoff of the impact of state firearm control laws (cross-sectionally) and of the federal Gun Control Act of 1968 (longitudinally) shows no significant effects on violent crime rates.

Handguns are involved in approximately 25,000 homicide, suicide, and accident deaths yearly, but because the American stock numbers about 54 million handguns, it is implausible that general measures of registration or confiscation can have more than marginal effects. From the perspective of enforceability, approaches that emphasize the banning of handguns are even less likely to succeed than did Prohibition because of the size of the weapons stock, strong incentives for non-compliance (self-defense, armed robbery), and overload on the criminal justice system (Don B. Kates, Jr.). A formal market analysis by Steven Balkin and John F. McDonald of stricter handgun controls, based on plausible assumptions, suggests that offender use of guns would be less affected than any other uses. In fact, there are good reasons for thinking that attempts to control handguns only (Kleck), or even more narrowly "Saturday-night specials" (Paula D. McClain), would push potential gun abusers into greater reliance on more deadly long (and cut-off) weapons, with potentially disastrous effects on fatality rates in assaults and robberies.

These are some of the key conclusions of the careful and rigorous analyses included in this volume. These are not the conclusions of "gun nuts," although Kates, a civil rights lawyer and former law professor, is on record as a strong opponent of conventional gun-control efforts on civil libertarian grounds. He claims not to have "asked . . . the positions of the other contributors

on particular firearms policy options" (p. 524). Some of them, including Wright, Kleck, and David J. Bordua, have previously favored gun-control legislation. The evidence adduced here, including reanalysis of past findings which seemed to support gun control, has led them to modify their policy preferences. The results do not, however, lead to uncritical endorsement of the "no controls" position. The high rates of interpersonal violence in the United States have socio-economic origins; the availability of weapons is largely incidental. The key to any effective attempt to reduce violence by controlling guns, Kates concludes, is to "disarm . . . persons who have a high potential for gun misuse" (p. 531), which in practice means people with some past involvement in violent offenses (Kleck and Bordua).

Historical and constitutional dimensions of the gun control debate are also addressed. The Second Amendment's protection of "the right of the people to keep and bear Arms" is shown by Stephen P. Halbrook to have origins in classical political philosophy and by Joyce Lee Malcolm to be firmly rooted in pre-nineteenth century British political tradition.

If the weight of evidence indicates that the conventional approaches to gun control are contrary to the American legal tradition and are likely to be counterproductive, the question remains why they continue to be advocated. Public opinion, which is often claimed to be strongly on the side of gun control, is reanalyzed by Bordua, who concludes from his comparisons of contending poll results that "The problem is more one of elite curtailment of a popular liberty about which the public is largely apathetic than of elite interference with a reform backed by passionate public desire" (p. 69). In a more speculative essay, Raymond G. Kessler summarizes the "political functions of gun control," but unaccountably misses its most obvious function in contemporary America: It is easy advocacy for public figures who want to appear responsive to an intractable social problem. The only strident chapter in the volume, by William R. Tonso, accuses the intellectual and social scientific supporters of gun control of "cosmopolitan ethnocentrism." The rejoinder is evident in other chapters: Some of the most competent scholars working on the subject have been persuaded by the evidence that unilateral civilian disarmament is neither a workable nor potentially effective solution to interpersonal violence in America.

On the whole, the 17 chapters in this volume make a sound and important contribution to the ongoing policy debate over firearms control. It complements and extends in important respects the recent benchmark study by James D. Wright, Peter H. Rossi, and Kathleen Daly, *Under the*

Gun: Weapons, Crime, and Violence in America (Aldine Publishing, 1983). The present volume gives further credibility to a conclusion from the earlier study (p. 322): "unless we solve the problem of interpersonal hatred, it may not matter very much what we do about guns."

TED ROBERT GURR

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Uninformed Choice: The Failure of the New Presidential Nominating System. By Scott Keeter and Cliff Zukin. (New York: Praeger Publishers, 1983. Pp. xx + 247. \$29.95.)

Widespread reforms since 1968 have significantly transformed the presidential nominating process. *Uninformed Choice* evaluates the voter's role in this new system, drawing upon survey data from the *New York Times/CBS* News exit polls, the 1980 Center for Political Studies Election Study, and the Eagleton Institute's New Jersey poll. The study answers three important questions about public involvement in nominations.

First, how representative are primary voters? Keeter and Zukin found each party's primary electorate higher in SES than the adult population but demographically and politically similar to partisan voters in general elections. Because of their disproportionate influence, representativeness in the earliest primaries also was examined. Voters here were just as unrepresentative demographically and were *politically* distinctive as well. What states begin the nominating process does matter!

Second, what is the quality of public thinking about the candidates? Keeter and Zukin found that many Americans possess only superficial knowledge of the candidates. In 1980 most voters could not identify the major candidates' ideologies (e.g., a bare majority could place Carter on the left-right scale; even fewer could locate Kennedy or Reagan) or their positions on the central issues (e.g., by September the average citizen could position Reagan on only 2.5 of four issues). Citizen knowledge was greatest about candidate personal traits. In public thinking, style dominates substance!

Third, how much does the campaign itself contribute to voter education? Public knowledge of the candidates increased little during the campaign, especially on issue positions and ideology. The learning that did take place largely involved candidate personal traits. Even when the long campaign is viewed as an educational process, style predominates.

Voters learned the most from the early contests, but they learned little more than that a candidate was a winner. Like (because of?) the media, the

public paid more attention to the "horse race" than the substance of the campaign. Indeed, so weakly anchored in an information base were preferences that the candidates' national popularity seemed to be conditioned by their fate in individual state contests. That the public is looking for a winner lends even more strategic importance to the sequencing of contests. Quite ironically, this also suggests that voters may share with party leaders a desire to put forward their party's most competitive candidate.

These disquieting results lead Keeter and Zukin to conclude that the current system for nominating presidential candidates is seriously flawed. They recommend changes, some of them implemented by the Democrats for 1984, to overcome these deficiencies. But all in all, their diagnosis is more impressive than their prescription.

Uninformed Choice is an important book, must reading for all concerned with our presidential nominating process. Yet the study has significant limitations. First, the role of group identifications in a voter's calculus is totally ignored. Second, the analysis is based on only two election campaigns, mostly 1980. The 1980 contest provides a test of voter knowledge under circumstances conducive to an informed electorate, because the major candidates that year were so distinctive and prominent. What might we find in other years? Third, as the authors concede, they study public opinion during the nominating process rather than voter behavior in actual contests. Their limited evidence on reactions to real primary elections suggests that voter choice may be more informed. Obviously we need information about voter choices at the polls rather than public opinion in the polls. Finally, real benchmarks need to be employed in evaluating citizen thinking during the nominating process. Are Americans less informed about candidates for nomination than about the major party nominees in the general election? If not, then Keeter and Zukin are indicting unmediated public opinion in general, not just the new nominating system.

PAUL ALLEN BECK

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Politics and Religious Consciousness in America.

By George Armstrong Kelly. (New Brunswick, N.J.: Transaction Books, 1984. Pp. xi + 312. \$29.95.)

Those who have read George Armstrong Kelly's provocative article on religion and politics in the Winter, 1982 *Daedalus* ("Faith, Freedom and Disenchantment," pp. 127-149) will be pleased with this elaboration on the motifs introduced

there. Kelly argues that the pathologies of American political institutions not only mimic the diseases of contemporary religious life, but also that "troubles in both spheres are more commonly related than is generally supposed" (p. 1). Conceding that the parallels are not always exact, he contends that the secularizing forces which have "reduced religion to privatized belief" are often the same sort "that challenge the profane community and the dignity of public life" (p. 30).

Although Kelly's work is especially timely, his analysis does not fit comfortably into any of the camps produced by the recent hostilities over the role of religion in politics. After delivering a much-deserved, if belated, slap at political scientists for their failure to take religion seriously, Kelly posits religion and politics as two distinct but overlapping control systems in human society. Although the domain of religion is the "sacred," and that of politics the "profane," the two systems are inevitably and constantly interacting, sometimes in tension and conflict, often in peaceful and fruitful collaboration.

Kelly examines this interaction in a learned and exceedingly subtle essay, taking off from reflections on American religion by observers as diverse as Tocqueville and Emerson, Alexander Campbell and Horace Bushnell, William James, and Robert Bellah. The topics covered are too numerous to detail, but several particularly valuable passages deserve mention: the analysis and critique of Tocqueville's view of the contribution of religion to civil liberty; the tracing of the "intricate and fragile" link between biblical prophecy and national eschatology; the delineation of parallel structures of "disenchantment" in modern religion and politics; and, the devastating critique of "civil religion," a notion so fashionable among American academics. Although Kelly's narrative will be most accessible to those of philosophic bent, the central theoretical contention and the many subsidiary themes invite—indeed, beg for—rigorous empirical investigation.

Kelly's conclusions, he admits, are "unbecomingly seditious in the company of Western humanism" (p. 31). Although secularization may have freed individuals from the bonds of tradition, it has not produced a public ethic superior to that of Judeo-Christianity. For Kelly, the idea of private morality so beloved by secularists is close to nonsense. Morality will always be "in politics," for it is "the perplexing minefield" in which we all live. "Traditionally, religion has animated it, law has given it a shell to inhabit, opinion has calibrated it, and politics has been domesticated by its force and persuasion" (p. 250).

Despite the inevitability of moral judgement—and hence, religion—in politics, Kelly is not optimistic that a new public ethic comparable to the

former Judeo-Christian one can be built in America. Today's mainline Protestantism, which contributed much to that earlier ethic, is so secularized as to provide the very model for the ailments afflicting both religion and politics. Sectarian fundamentalism is more vigorous, but alternates between creating "multiple moral territories" and complete privatization. Even American Catholicism, with a much longer tradition, "has embarked on its own versions of privatization and secularization" and is not a likely source for revival (p. 260).

Kelly is convinced, however, that "deliberate architects" will be required to rummage in the ruins of American religious and political traditions to find the materials to reconstruct a public ethic. Although Kelly does not begin this work himself, readers of this volume will be grateful to him for clearing away much of the debris and revealing at least some of the old foundations.

JAMES L. GUTH

Furman University

The Regulation of American Federalism. By Donald F. Kettl. (Baton Rouge: Louisiana State University Press, 1983. Pp. xviii + 195. \$20.00.)

This thoughtful and timely book examines the federal government's growing regulation of our subnational governments. Kettl defines his basic question as "which piper calls the tune when one government raises the money and another spends it?" (p. 154). He examines the relationship between fiscal dependence and political independence generally within the Community Development Block Grants program as a case study.

Kettl defines regulation as "a dynamic process with the potential for substantially altering the distribution of money and power, a political process in the largest sense of that term" (pp. 22-23). Regulation as the principal feature of federal attempts to control intergovernmental grant programs emerged in the 1970s after attempts to control by category of grant and control by application during earlier years. Although an individual regulation may have a laudable goal, the package of regulations—particularly cross-cutting rules that apply across the board to many programs—have created four burdens for state and local governments: high financial costs for compliance and creation of new agencies to keep records and prove compliance, uncertainties in planning projects, adversarial proceedings among governments, and token compliance to meet terms of the rules without conforming with their goals (pp. 8-14).

State and local governments are described as

"uncertain brides in the partnership of American federalism" (p. xv) who along the way collected something old, new, borrowed, and blue. In turn, these items are: sincere but not always effective attempts to streamline regulations, the strengthened role of the Office of Management and Budget in dealing with regulations, the use of block grants to loosen federal controls, and new programs that create "too many responsibilities supported with too little money" (p. 140).

Reagan, according to Kettl, is the "first American president to succeed in firmly establishing a public image of states and cities as regulated governments" and with success in "creating a new partnership" (p. 130). Reforms are difficult because the number of players in the political process of regulation has increased and includes an influential track dominated by professionals hidden from public view and a public track that is open and less important.

The heart of the problem for Kettl is that wider uniformity of federal regulation robs state and local governments of the discretion the programs promised. There are no uniformly good and bad guys in the study, but Kettl does spend more time on the results of regulation rather than on its causes.

Kettl sees the process of regulation as fitting naturally in the political process of federalism because choices have to be made between uniformity and diversity. In his examination of the decentralization alternative to centralized regulation, Kettl believes a recasting of the categories of federal aid would be a worthy stopgap reform, because 90% of federal grant programs account for only 10% of the money. Improved administrative mechanisms for reviewing state and local compliance with rules is a more basic reform, and more responsibility should be decentralized to field offices. Although these reforms neither will be easily achieved nor prove the solution, Kettl concludes that "Only ironically by renouncing the obsession with controlling administrative detail can federal control over those matters truly of national importance be strengthened" (p. 171).

This volume is one in a series on the American presidency sponsored by the Miller Center of Public Affairs at the University of Virginia. It is a worthy study and reflects an original approach to the problem of reconsidering the need for effective central leadership with the constitutional imperatives of limited government and divided but shared power. Kettl's analysis of the problems of regulation is more discerning than his approach for alleviating them, but the book has a message that deserves a wide and thoughtful audience.

GEORGE S. BLAIR

Claremont Graduate School

Agendas, Alternatives, and Public Policy. By John W. Kingdon. (Boston: Little, Brown, 1984. Pp. ix + 240. \$7.95, paper.)

How the national agenda is set is the most complex and least understood aspect of the policy-making process. Kingdon's volume is an important contribution toward a better understanding of this important subject.

Kingdon's study is based on 247 lengthy and detailed interviews with congressional staff, executive branch political appointees, and upper-level civil servants and presidential staff in 1976, 1977, 1978, and 1979. His attention is primarily devoted to two domestic policy areas, health and transportation. Faculty grants, a grant from the National Science Foundation, and a John Simon Guggenheim Foundation fellowship supported the research and writing effort. The interviews form a panel study, with tracking of the interviews and the content of the agendas over time. A snowballing approach was applied to expand the sample to reach influential or top decision makers in the health and transportation policy fields. Kingdon also conducted extensive research of government documents and written materials focusing on 23 cognate case studies in the health and transportation fields. Kingdon wisely concentrates on two policy areas, the breadth of each being substantial enough to convince me that he can make accurate generalizations about much of domestic policy-making. Moreover, the interviews and the case studies provide a formidable and convincing data base on which to develop this study.

Kingdon found that, although no one set of actors dominates any phase of the agenda-setting process, the president and his appointees have substantial influence in agenda-setting; Congress is effective in agenda-setting and alternative specification (the selection of alternatives from which the choice is made); interest groups, academics, researchers, and consultants tend to be influential at the alternative specification phase; and the media are less influential than expected, although they appear to have impact on public opinion, which in turn influences politicians.

Kingdon provides an insightful chapter (4) on theories of the policymaking process and then develops his own revised model. He concludes that the federal government "is seen as an organized anarchy," with more emphasis on "organized" than "anarchy." The remaining five chapters of his book provide convincing support for this theory. Kingdon observes three major "process streams" in federal policymaking: problem recognition, the formation and refining of policy proposals, and politics. These operate independently of each other and can be best understood by patterned (systematic) and sometimes

random events that open and close policy windows that link the various streams and couple agenda issues.

Kingdon has greatly improved our understanding of the policymaking labyrinth. This work is a major contribution to this field. This study is appropriately grounded in quantified field research, and it is thoroughly and exhaustively researched. The volume is well organized, lucid, concise, scholarly, well edited, and delightful to read.

CLIFFORD J. WIRTH

University of New Hampshire

The Heyday of American Communism: The Depression Decade. By Harvey Klehr. (New York: Basic Books, 1984. Pp. xiv + 511. \$26.50.)

Harvey Klehr's massive book fills a long-standing gap in the history of American communism. As part of a large-scale study sponsored during the 1950s by the Fund for the Republic, Theodore Draper published two brilliant volumes, *The Roots of American Communism* (Viking Press, 1957) and *American Communism and Soviet Russia* (Viking Press, 1960), which detail the history of the Party through the 1920s. He then abandoned the project and, although other studies have appeared dealing with the 1940s and 1950s, the story of the critical decade of the Great Depression has not been told in detail.

Fortunately, Draper deposited his valuable collection of research materials, plus drafts of several early chapters of a projected work on this period, in the library of Emory University. Building on this base, and adding much original research of his own, Klehr has provided an appropriate successor to Draper's now classic studies.

The history of the period falls into two parts, with a transitional stage coming between. The first phase, notable for its revolutionary radicalism, extended from 1929 to 1934 and is referred to in the histories of communism as the Third Period. The second phase ran from 1935 to 1939 and encompasses the more widely known Popular Front.

The book provides detailed accounts of Communist party activity among the unemployed and the intellectuals, the youth and blacks, agricultural workers, and in such states as New York, Minnesota, California, and parts of the South where its political success was greatest. Klehr says little about such episodes as the infiltration of Hollywood or the activities of the Abraham Lincoln Brigade in Spain on the grounds that they have been extensively written about elsewhere. Students of American political thought might hope for more about the intellectuals, but they

too have been written about at length, and Klehr handles this important subject very well given the space limitations of an already large book. Although he does briefly discuss the underground life of the Party, there is little here about espionage activities, and one wonders if this reflects Klehr's assessment of the relative importance of this topic which was to become so bitterly controversial during the 1940s and 1950s.

This book contains great quantities of detailed information. It is very well documented and possesses an outstanding index. Although a formal bibliography of the published sources used would have been useful, these qualities will make this book an invaluable reference source.

The Heyday of American Communism will no doubt remain the standard work on its subject for many years to come, although veterans of the movement may see some things in a different perspective. Thus, Hal Draper, in the course of a generally laudatory discussion, finds Klehr to be weak on the Third Period in general and to misunderstand the impact of the Franco-Soviet pact of 1935 in particular ("Pie in the Sky," *New York Review of Books*, May 10, 1984, pp. 25, 28-31). Nonetheless, the factual detail Klehr has amassed make this a very useful effort.

However, the sheer volume of information suggests the book's major weakness. The study is largely descriptive and lacks an interpretive framework adequate to the complexity of the subject. The history of the American Communist party in this period is part of the often bizarre history of Stalinism. Stalinism is in need of explanation—of a theory. Without such a theory, one can be swallowed up in detail. For such a framework, the prospective reader might well turn in advance to the brilliant and suggestive concluding chapter of *The American Communist Party* by Irving Howe and Lewis Coser (Da Capo Press, 1962). Such a perspective will help the reader to extract even more from Klehr's labors.

JAMES P. YOUNG

State University of New York, Binghamton

Money and Politics in the United States: Financing Elections in the 1980s. Edited by Michael J. Malbin. (Chatham, N.J.: Chatham House Publishers, 1984. Pp. 324. \$12.95, paper.)

The conventional wisdom on campaign finance, particularly that held by journalists, is that political money threatens democracy by corrupting officials. Scholars who study campaign spending find, to the contrary, that political money furthers democracy by increasing electoral competition. In this vein, *Money and Politics* con-

cludes, one might say, that all money tends to root out evil.

Money and Politics responds to most of the complaints made about campaign finance. The most common critique is that elections cost too much. Herbert Alexander rebuts this charge in his survey of 1980 presidential campaign expenditures, concluding that campaign spending supplies necessary information to voters. In this regard American elections are underfunded, although less so than in the past. However, well-intentioned regulations often produce expenditures that provide less and less helpful information. Ruth Jones's survey of campaign finance in the states, and Richard Smolka's discussion of recent court cases, arrive at similar conclusions.

The fear of large expenditures results from the assumption that those who spend the most win elections. Gary Jacobson demonstrates the inadequacy of this assumption. In recent congressional elections, increased spending by incumbent candidates did not improve their performance at the polls; however, the more money challenging candidates spent, the more votes they received. Spending by both kinds of candidates mattered only in very close elections. Here funds from PACs and parties (particularly the Republicans) helped marginal incumbents stay in office. However, it was money from PACs and parties that made these elections close in the first place by funding challengers.

Fear of political money often stems from disapproval of those who provide campaign funds. Various critics dislike, in turn, well-financed parties, affluent interest groups, and well-heeled ideological PACs. *Money and Politics* contains good analyses of each of these sources of funds. David Adamany argues that campaign finance has helped create a strong, centralized, and professional party for the Republicans; the Democrats are attempting to emulate their success. The chief beneficiaries of strong parties are weak candidates, those most in need of financial assistance. But because of the funds supplied by PACs, parties do not dominate campaign finance.

PACs often reinforce the tendencies of the parties, however. Theodore Eismeyer and Phillip Pollock find that interest group PACs are very diverse, and for the typical PAC, ideology is as important as self-interest. Although much PAC money goes to incumbents, PACs with ideological concerns contribute to many challengers. Margaret Latus finds that ideological PACs behave similarly. Left- and right-wing PACs imitate each others' strategy and tactics, including the strong backing of candidates in close elections.

Unfortunately, this volume does not include a study dealing with the allegation that money buys votes in Congress. Here too, the weight of schol-

arship suggests that money rarely influences congressional behavior, and when it does, it challenges the established order. Several of the above authors arrive at this conclusion indirectly, however.

Collections of essays are often disjointed, but thanks to Michael Malbin's excellent introduction and concluding essay, *Money and Politics* fits together well. Malbin deals principally with proposals for campaign finance reform, unmasking the notion underlying much of the foregoing criticism: Special interest money corrupts politics. He argues the opposite: Special interest money is an unavoidable, and indeed desirable, aspect of politics. Limiting political money will not hinder the contacts groups have with officials, only their electoral activities, particularly the desirable funding of risky candidacies.

Money and Politics is a well-conceived and executed book. I highly recommend it, particularly to political journalists.

JOHN C. GREEN

Furman University

Changing U.S. Military Manpower Realities.

Edited by Franklin D. Margiotta, James Brown, and Michael J. Collins. (Boulder, Colo.: Westview Press, 1983. Pp. xviii + 267. \$25.00.)

Paradoxes of Power: The Military Establishment in the Eighties. By Adam Yarmolinsky and Gregory D. Foster. (Bloomington: Indiana University Press, 1983. Pp. ii + 154. \$15.00.)

The two books under review utilize significantly different approaches but pursue a parallel course of attempting to evaluate the current U.S. national security system and its future in the 1980s.

In 1971 Adam Yarmolinsky published a sound analysis, *The Military Establishment: Its Impact on American Society* (Harper & Row). *Paradoxes of Power* is a recent revision, a well-written, concise overview of U.S. military policies that contrasts what Americans expect of their defense establishment and what the system delivers. The fact that the military fails to provide what is expected is not always their fault. Society's expectations often are unrealistic, and the American people through Congress do not always provide the capabilities for the military to meet these expectations.

This is a good book for the layman because, as Yarmolinsky notes, the military establishment "is so heavily and extensively involved in almost every aspect of American life that we cannot understand our own society without learning something about . . . this elemental component"

(p. 2). In subsequent chapters the book examines the military impact upon and interaction with government, the economy, and society. Concluding chapters examine arms control issues and four alternate scenarios of the future.

Four "paradoxes" underlie this analysis: the need to deter because defense in the nuclear age is impossible, the inability of superpowers to use the very force that made them such, the conflicting basis of civilian-military control, and the politically volatile debate between the best approach to peace—through enhanced security systems or through arms reduction.

The text provides a pragmatic, general discussion of how a thoroughly knowledgeable participant-observer of the Washington scene would explain to an outsider how government really works. There is very little new material here, but the book is well conceptualized. Yarmolinsky and Foster provide tantalizing snippets of issues, but without close examination or documentation. Perhaps this is necessary in a brief overview text, but the superficial treatment can be frustrating.

Changing U.S. Military Manpower Realities is principally a collection of in-house studies by scholars within or closely related to the military educational system. The first three chapters are the core of the text: the fundamental manpower availability issues and the impact upon defense planning. These issues grew out of the 1977 Conference on Changing Military Manpower Realities, which concluded that military manpower is the single most pressing defense issue in the 1980s and 1990s.

The theme of these chapters is that the All-Volunteer Military Force is not working in its present form, but the alternatives are costly. To Margiotta, this is due primarily to an identity crisis within the military and a nationwide "declining sense of the legitimacy of military service" (p. 13). This makes it difficult for Americans to accept the values, norms, and sacrifices of military service in peacetime. Similarly, "the average American finds it difficult to understand and support [arms limitation] and then commit himself or herself psychologically and emotionally to national defense" (p. 14).

The remaining chapters address a variety of manpower issues including recruitment and retention for ground forces, reserve forces, pilots, minorities, and women. The impact upon individuals and their families is examined, followed by a thoughtful concluding chapter by Morris Janowitz on the role of the citizen-soldier in a democracy.

The issues are valid, important, and generally well developed. The pessimistic conclusions of most of the book's manpower projections, however, have not proven entirely accurate because,

as Margiotta notes, "One must be cautious about predicting social trends." Precisely! The manpower projections of the late-1970s, based upon earlier standard demographic measures, did not materialize in the early-1980s because of shifting attitudes in America. Many of the dismal forecasts simply did not occur. Nevertheless, the problems will persist and cannot be ignored by the public.

The manpower studies are well documented, but principally from government or related sources. Some of these studies used invalid methods or assumptions, and so the conclusions in some cases must be questioned. The 1970 Gates Committee and the 1976 Defense Manpower Commission reports are somewhat unsophisticated and certainly outdated. The gloomy forecast for "the Orwellian year of 1984" (p. 11) seems out of place now that 1984 has come and gone. Like *Paradoxes of Power*, this text suffers from occasional technical errors and inaccuracies.

Paradoxes of Power is a thoroughly useful pragmatic overview that will survive for several years in the classroom. *Changing U.S. Military Manpower Realities* has a valid theme and useful data, but suffers in its ability to play the dangerous game of forecasting.

ABBOTT A. BRAYTON

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Federal Court Caseloads. By William P. McLauchlan. (New York: Praeger Publishers, 1984. Pp. xiv + 219. \$25.95.)

Federal Court Caseloads is a somewhat unconventional social science product. Although it might be tempting to search for causal variables that explain and predict the increasing demands that have been placed onto U.S. courts, McLauchlan's more modest purpose is to describe the growth of case filings in the federal court system. He deliberately has shied away from such a study and from statistical techniques which may or may not have borne that empirical fruit. He acknowledges that "description is not widely accepted as an analytic technique" (p. 17). Moreover, he employs exploratory data analysis (EDA), a statistical tool which might be unfamiliar to some scholars who are comfortable with traditional statistics. But EDA is most useful to detail the findings of this narrowly focused subject matter, because it "cannot be used for testing hypotheses in the traditional, confirmatory fashion" (p. 21). Scholars might at some future time employ more traditional, hypothesis-testing data analysis and answer some of the "un-

answered and interesting questions about court caseload" (p. 17) that appear in this work.

The book discusses separately the growth in case filings for each federal court level. It then draws some parallels between the district courts, U.S. Courts of Appeals, and the Supreme Court with regard to caseloads, case terminations, and pending cases. Some important descriptive data are discussed. For example, viewed from 1880 to 1980, Supreme Court caseloads have grown most rapidly only since World War II. McLauchlan refers to the mid-1940s as "perhaps the primary . . . factor in changing the caseload pattern" (p. 71). Plots of Supreme Court caseloads by decade, by chief justiceship, and by petition categories uncover no different trends.

McLauchlan shows that caseload growth has occurred in the last 15 years for the U.S. Courts of Appeals. He notes that the Fourth Circuit "has faced . . . a high proportion of civil appeals" (p. 108). This is surprising. Other circuits, the Second, Fifth, and Ninth, have major population centers that the Fourth lacks. McLauchlan displays other descriptive data for the circuits by judgeship, by three-judge panel, and by case type.

To explore the categories of cases likely to be found in district courts beyond aggregate data reported, McLauchlan shifts his concentration to an admittedly "non-random set of districts . . . [for] an intuitive examination" (p. 147). He looks at case filings in nine districts for the period from 1977 to 1981. Not surprisingly, he finds that "the causes of case filings are probably so idiosyncratic with each district that predictions or forecasts of future patterns have to be based on district-specific variables" (p. 164). Generally, after recent growth, district court case filings may be stabilizing currently.

McLauchlan notes that although "cases drop out of the system quickly and early in the process, . . . [courts] have to be ready to make that investment" (p. 199) of time and resources. He reminds us that due process of law is labor-intensive, and "even the frivolous suits must be treated seriously by the court" (p. 199). But few case filing patterns are easy to understand. For example, increases have been seen in reverse hierarchical order, "with the increase in the courts of appeals coming over ten years after the Supreme Court increase, and the district court increase appearing some five years after that" (p. 207). Elsewhere, the appeals circuits are "operating with increasing efficiency" (p. 197) in terminating cases, yet "have suffered the greatest increases in pending cases" (p. 197). In the final analysis, McLauchlan cannot suggest that "the most general kinds of influences—population, economic performance, or social settings—can be labeled as *the* causes of caseload" (p. 207).

The simple political solution to caseload growth calls for more judgeships at the district and appeals levels. Additionally, jurisdiction reforms such as purging the federal courts of diversity cases is often suggested. Interestingly, McLauchlan finds that for the district courts (and the appellate circuits) the number of diversity cases is dwarfed compared with federal question cases. The Supreme Court may be facing an "insurmountable" (p. 204) problem that no politically feasible reform can solve. The High Court appears to be "functioning at full capacity" (p. 196), and case filings have increased at a rate greater than the other two court levels.

Learning to read the book's unconventional EDA boxplots and stem-leaf diagrams takes some time, even for the experienced empirical student who is yet a novice to exploratory data analysis. McLauchlan is correct to warn the reader that the methods chapter describing the technique may not be enough and that further reading might be required. That caution aside, *Federal Court Caseloads* provides research hints that may sustain judicial process and court administration scholars. Future, more detailed studies will explain and predict caseload patterns both at the macro- and micro-level.

DAVID S. MANN

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The Policy Organization. By Arnold J. Meltsner and Christopher Bellavita. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 253. \$25.00, cloth; \$12.50, paper.)

This work grew out of the "Managing Information" series dealing with organization studies and decision making. It is Volume 5 of the series edited by Aaron Wildavsky, which concerns itself with information overload that managers and policymakers alike must deal with in a period of finite resources. The California State Department of Education pupil proficiency assessment program is used as a singular case study to illustrate the proposed design. Intended for managers and their policy analysts, the book is a collaborative effort of both groups to try to understand each other.

The first chapter focuses on communication and effective policy management. Chapter 2 places these factors in an organizational theoretical context, using a checklist of eight major categories that are basic fundamental administration.

The succeeding chapters are devoted to the individual accounts of the case study. The authors' approach of organizational categories is applied

as a way of clarifying policy decisions and the operations of existing organization. Each chapter studies in depth proficiency assessment, organizational goals, organizational membership, the environment, the structure or design for change, resources, tasks, decisions, and communications. Each of these categories is viewed as a design element. The authors conclude that policymakers are influenced when the research is of high quality, uses specific empirical questions of modest scientific complexity, and is provided by prestigious researchers who have credibility with decision makers.

However, it is inappropriate to use this approach in every case. For example, such a scheme should not be utilized on technical problems that have obvious technical solutions with few social or organizational implications or in a symbolic way such as initiating a study to postpone action. Although the approach of applying a list of organizational categories to illumine policy decisions and the operations of existing and proposed organizations can be useful, it is not designed to advance the science of administration. Rather it is intended to enlarge the vision and understanding of the individual manager and analyst.

Due to its anecdotal and storytelling approach, this study may be classified as a purely descriptive "how to" technique manual. It is a guide or reminder of what should be considered in dealing with organizational description. It appears to be a method to use, but guarantees no final success or possible solutions. It is how you get there that counts. The book relies on simple and basic fundamentals, using an elementary approach. The case study is interesting, but it isn't unique. Although proficiency testing almost sounds like a discipline or skill, no attempt is made to make it universally applicable. As a factual matter, the design elements surface as common sense. Witness the following statement from page 221 describing how to use the design elements:

We do not expect that many managers will want to do the work of describing their organizations themselves. Indeed, if they want an independent view of their organization, then they are better off not doing it themselves. If they did, they would see the organization through their own preconceptions and because of status differences would not likely get a straight story from members. . . . Ideally the manager should choose an outside person to undertake an organizational description. That person need not be a consultant to be considered an outsider just as long as the person is truly outside the boundaries of the policy organization.

Despite its simplicity, *The Policy Organization* has a good overview and discussion of organiza-

tions, a helpful bibliography, and would be useful to the uninitiated who wish to learn about organizational processes and description.

FRANK T. COLON

Lehigh University

Making Something of Ourselves: On Culture and Politics in the United States. By Richard M. Merelman. (Berkeley: University of California Press, 1984. Pp. xii + 274. \$15.95.)

This book asserts that the United States has "evolved . . . a loosely bounded culture" which undermines the democratic promise in America because it gives Americans "little real purchase on the massive, hierarchical political and economic structures that dominate their lives" (p. 1). This loosely bounded culture, Merelman argues, results in part from the decline of the Puritan, democratic, and class visions in America.

These cultural visions have common characteristics. First, they provide identities to their adherents and offer a means by which to understand and interpret society. Second, these visions mesh individual identity with group membership and stimulate organized group action. Third, they describe ways that groups can attain control of the structure of power in society. According to Merelman, the Puritan vision "bound together individual, church, community, state and God in a joint enterprise" (p. 6), but declined because the integral role it gave intolerance choked its supply of new adherents and because it collided with political theories that grounded individual rights and the state in non-Biblical justifications. The democratic vision, emphasizing tolerance as an imperative allowing the inclusion, "first under procedural guarantees . . . and then under substantive policy agreements" (p. 10) of diverse social groups, may not, Merelman notes, have ever been particularly powerful. He finds that it is much less compelling now than when first codified by political scientists in the 1950s and 60s. The weakness of class consciousness in America has cast doubt on the power of the class vision.

Merelman also discusses the roles of the frontier, the weakening of deference to age, the demise of the three-generation family living unit, ethnic immigration, and the automobile in sapping the power of those visions that give individuals an identity—often nonvoluntary and inescapable—as part of a group. Americans are left, then, with a culture that makes the liberated individual the basic cultural unit (p. 30) and leaves individuals to "make something of themselves" without help from cultural visions. Such individuals are freed from group identities—as

members of churches, unions, ethnic groups, and so on—that establish boundaries between persons of different groups. Individuals are consequently propelled into a world of personal relationships in which the values of “sentiment, rashness, spontaneity, equality, and transient cooperation” (p. 43) are key to getting along. Liberation from groups and their tight boundaries has, Merelman claims, a cost: It damages Americans’ ability to create cultural visions that empower them for political action in a public world dominated by “reason, prudence, calculation, inequality, and unending competition” (p. 43).

The first two chapters of this book, which are devoted to developing this view of cultural visions, are strong ones. They present an argument that will interest and challenge political scientists. The three following chapters, given to discussing how loose-boundedness is reflected and reproduced by television, advertising, and education, may be of less interest. They are laden with anthropological examples and anecdotes. Still, these chapters make their point: As refiners and purveyors of loose-bounded culture, television, advertising, and education give a deceptive view of America and encourage an “abstract individualism” that uproots the individual from the groups that shape each person’s identity.

Merelman concludes his analysis by considering whether the American polity can play a positive role as a cultural center that encourages citizen-empowering cultural visions. His findings—based on case studies of two government efforts in the field of culture creation—are not encouraging. In both cases, it is evident that American antipathy toward this role for government fosters resistance to its creations, and thus perpetuates loose-boundedness. And as loose-boundedness is perpetuated, so is the political impotence of the isolated abstract individual in America.

Although this book employs some slippery concepts and definitions, it makes an important contribution to our understanding of the cultural bases in America of political inaction and powerlessness.

W. RICHARD MERRIMAN

Berea College

Presidential Popularity and the Economy. By Kristen Renwick Monroe. (New York: Praeger Publishers, 1984. Pp. xxiii + 289. \$23.95.)

Many political analysts in 1980 felt it was the public’s reaction to Ronald Reagan’s rhetorical question about the economy—“Are you better off now than you were four years ago?”—that cinched Reagan’s electoral victory over incumbent

president Jimmy Carter. In other words, of all Carter’s political weaknesses, it was the personal effects that his economic mismanagement had on individual Americans that finally moved a largely uninspired and undecided electorate to put the nation on a new, rightward course. If I understand Kristen Renwick Monroe’s arguments correctly, however, it would be a mistake to hold too strictly to this interpretation of the 1980 election. A relationship between an incumbent president’s popularity and the state of the economy does exist, but within a complex matrix of qualifiers raising important questions about the depth of rational, pocketbook calculations by voters that Reagan’s questions to the public presupposed.

I must state my caveat about understanding Monroe’s arguments correctly because I found, as I believe many readers will find, those arguments presented with a certain lack of focus and pointedness. *Presidential Popularity and the Economy* as a title refers to much less than what the book attempts to deal with, and it is not quite the subject toward which readers will find themselves oriented at the end. Rather, the full range of Monroe’s concerns includes: the theoretical tension between economic models of human behavior (emphasizing rational intentions) and sociological models (emphasizing prerational dispositions); the accuracy of cyclical models of presidential popularity versus models stressing popular reactions to specific acts and events; the validity of the concept of sociotropic (or collectively oriented) voting; and the influence on presidential popularity of such key variables as partisan ties and social class. Trying to balance the findings she derives from pursuing all these concerns leads Monroe to the almost inevitable conclusion that the determinants of presidential popularity can best be understood through an “integrated rational model [that] incorporates critical nonrational factors in an analytical framework essentially rational in orientation” (pp. 87-88). Her closing words deal with her conviction that the development of this new analytical model to overcome the tension between the economic and sociological characterizations of behavior “will be the next theoretical breakthrough in political science” (p. 204). Ultimately, the topic suggested by the book’s title is overshadowed by reflections and recommendations dealing more with the theory and methodology of behavioral analysis in general.

Monroe’s suggestion that a necessary breakthrough synthesizing conflicting behavioral models has *yet* to occur may be partial explanation for the problems left behind by what she does say specifically about presidential popularity and the economy. She asserts that economic conditions and expectations play a primary role in shap-

ing the public's evaluation of presidents—the pocketbook voter has validity as a conceptual norm—but important constraints on this role must be recognized. Indeed, these constraints are so important that one may worry about the exceptions outweighing the rule. Monroe's central example is the "partisan filter" through which voters pass their expectations and perceptions, with some remarkable effects on their rationality. Monroe's data suggest, for instance, that the popularity of Democratic presidents increases as unemployment *increases*. Her too brief speculation is that this occurs because people view the Democratic party as most likely to solve the unemployment problem, even while it persists during a Democratic administration (pp. 175-176). Thus, to return to the example of the 1980 election, the implication is that Jimmy Carter might have had a better chance if a few more unemployed voters had been available to hear Reagan's query about personal well-being.

Explaining this kind of anomaly and other questions raised by the excellent opinion data assembled by Monroe (including previously unpublished Gallup poll data) is what should have been the more intentional thrust of this book, with less discussion of competing behavioral theories proposed by other scholars and the attendant methodological debates. If such explanation had been held at the center of Monroe's focus, she might herself have broached the theoretical breakthrough that she instead leaves us to ponder.

DAVID MENNINGER

University of California, Los Angeles

Making an Issue of Child Abuse: Political Agenda Setting for Social Problems. By Barbara J. Nelson. (Chicago: University of Chicago Press, 1984. Pp. xiv + 169. \$17.50.)

During the 1970s, policy-oriented political scientists focused primarily upon the implementation stage of the policy process. They attributed policy successes and, more likely, policy failures to implementation. Recently, political scientists, including Barbara J. Nelson, reexamined the agenda-setting process. Nelson's *Making an Issue of Child Abuse* provides a welcome addition to this literature.

Even a cursory reading of newspapers or watching of television stimulates one to question why child abuse is now considered a public problem. It is clear that child abuse and neglect occurred both in previous times and in other nations. Nelson asserts that "a social problem is a social construct" (p. 5). Thus, a social problem is not solely an empirical reality but is defined by organized in-

dividuals who wish to solve the problem and a society that recognizes it.

The natural rights ideology, civic education, and the rise in the bourgeois family created "protected childhood" beliefs in nineteenth-century America. These beliefs, the rise of protective societies, and media attention to cases of child maltreatment stimulated, at first, child rescue policies and, later, family rehabilitation policies. In the 1960s, society rediscovered child abuse due to equity beliefs, the research of radiologists and, later, pediatricians, and the media coverage of that research.

This book centers on the agenda-setting actors and processes from the 1960s to the 1980s. The Children's Bureau established child abuse on its agenda at the urging of physicians and the American Humane Association. This fortuitously occurred simultaneously with an uncommitted and large increase in the Bureau's research budget. With the new money, the Bureau established "a network of researchers whose careers were enhanced by child abuse research . . . [keeping] child abuse on its agenda and the public's agenda, in an ever-widening circle" (p. 47).

The media followed the research results, creating more news stories. In addition to this, the linkage with broader social issues, more issue differentiation, and soft news stories supported a longer than usual issue attention cycle for child abuse. The media attention, interest group activity, and the model law drafted by the Children's Bureau caused every state to pass child abuse reporting laws between 1963 and 1967. This rapid diffusion was due to "the symbolic and valence qualities of the issue, and the absence of monetary or political costs. . ." (p. 80).

On the national level, Senator Walter Mondale introduced the first adopted legislation on child abuse. Earlier, the Comprehensive Child Development Act, supported by Mondale, was defeated in Congress. But Mondale discovered the key to legislative success: "Not even Richard Nixon is in favor of child abuse" (p. 102). A symbolic issue supported by universal values, high public salience, low conflict, and a willingness to ride herd, produced the success that Mondale needed to both pass the legislation and appear candidate-like in his unannounced campaign for the presidency. By 1981, however, the perceived threat to family autonomy and Reagan's desire for block grants nearly killed the program.

In a moment of broad theoretical thrust, Nelson suggests that child abuse went from a valence to a controversial issue because the policy had the tensions of liberal reform. In order to obtain serious consideration, a problem must be defined as deviant, individual, and psychological with, hopefully, a medical solution. "The

scenario of having done too much and too little is one consequence of liberal reform" (p. 137).

This book has significance beyond the issue of child abuse. Nelson considers the issue cycles, the role of the media, the type of issue, the structural and individual factors in agenda-setting. This makes a neat case study with much theoretical import.

DAVID C. COLBY

University of Maryland Baltimore County

The American Presidency. By Benjamin I. Page and Mark P. Petracca. (New York: McGraw-Hill, 1983. Pp. xiii + 418. \$13.95, paper.)

Benjamin Page and Mark Petracca set out to do two things in *The American Presidency*. The first is to write a comprehensive text; the second is to assert several arguments of their own about the power of the presidency.

The authors have clearly succeeded in their first objective. Their diligence has resulted in a thorough text that covers the major topics considered in comparable works, plus a few that are often neglected. For example, they discuss the nature and features of political power early in the book and put the main themes of this discussion to good use thereafter. They also provide an organizing and explanatory device in a systems model of presidential power.

Their model puts the president amid interacting governmental institutions that respond to international, social, economic, technological, and demographic needs as interpreted and mediated by public and group activities. In responding, the president, Congress, and, to a lesser extent, the bureaucracy and courts serve to affect policy outcomes. The elements of their systems model supply and justify the book's major topics and thereby insure its comprehensiveness. Interest groups and courts, for example, are given the attention they deserve, whereas these are often slighted in other works. Also included is a chapter on the "Evolving Presidency," which discusses every president rather than just the "strong" or recent ones. One area that is somewhat underdeveloped involves the different models of information gathering and decision making, especially as these relate to presidential isolation, responsiveness, and control.

The second purpose of the book—that of pro-

viding new information and a set of perspectives on presidential power—is what distinguishes this work from most texts and makes it interesting to scholars. The authors argue that there is substantial truth to the view that the presidency is the most powerful office in the world as judged by its proximate influence on policy. They note, with many others, that such power has generally increased, albeit erratically over time, and that it varies across issue and policy domains. Although there is no news in that, Page and Petracca add to what is usually reported by supplying a rough hierarchy of presidential power in several domestic policy areas (e.g., social welfare, civil rights, macroeconomics, and public works).

There is, however, an all-important qualification to the authors' assertion of presidential strength. Their systems model serves to illustrate their premise that environmental factors, not presidents, are the ultimate cause of policy outcomes. Despite the president's proximate discretion, the presidency is essentially a conduit of societal needs made compelling primarily by the actions of the public and of groups.

Because of this view, Page and Petracca reject the "great man" perspective of Wilson, Rossiter, Pious, and others and generally deemphasize personality, whim, and individualism. Similarly, they reject the "bureaucratic politics" model that pictures the presidency as the central policy initiator and broker among competing institutions (à la Neustadt, Allison, and Halperin). Democratic and pluralistic pressures generally determine presidential initiatives and basic policy outcomes. Thus, they conclude that the presidency is quite powerful as regards specific policies, but quite weak as a determinate of continuity and change in basic governmental directions.

There is much to be said for this book aside from its substantive thoroughness and conceptual clarity. While essentially a text, it does take sides in scholarly debates. It also supplies some original research, as in its discussion of the congruence between policy and public opinion. Although systems analysis is sometimes considered trite these days, it can be used effectively as a heuristic device. In this book it helps clarify major themes and contributes to a generally logical, well-justified, and smooth presentation. The book is written in lucid, standard English with little reliance on jargon despite the systems approach and an infrequent reference to the president as an "intervening variable."

Page and Petracca have added new details to the outlines of what we already know and have argued convincingly in support for their perspectives. Thus, they have surpassed their goal of providing a comprehensive text. They have also taken large strides toward their second goal of providing

scholarly perspectives and new information on the American presidency.

JAMES A. DAVIS

Oklahoma State University

America's Unwritten Constitution: Science, Religion, and Political Responsibility. By Don K. Price. (Baton Rouge: Louisiana State University Press, 1983. Pp. xvi + 202. \$19.95.)

According to Price, the inability of accountable leaders to develop coherent policies and programs is at the heart of a continuing problem created by ingrained national prejudices found in America's unwritten constitution. He traces the origin of these attitudes to John Calvin and the Puritan founders. There is no system of political authority in existence with the capacity to effect a rational comprehensive policy or to carry out a coherent program. Power is dispersed, no single point of responsible control over the formulation of general public policy is to be discovered, and no long-range plans or policy priorities are set and followed. Constitutional reforms that imitate the parliamentary cabinet system are required to remedy this problem. Price provides the layman with a clear, concise summary of the status quo in administrative matters. These current difficulties, according to Price, are compounded by the continual conflict between Congress and the president over control of the administrative branch. Congressional subcommittees assisted by professional staffs control specialized program administration. Bureau chiefs acting in collusion with congressional subcommittees undermine the authority of the president and departmental secretaries. Further dispersal is caused by the power of special interest lobbyists, which accelerates the disintegration of party discipline.

The central message in this book is that the president should be delegated greater power to manage the formulation of general policy and to control the administrative branch. Price asserts that the administrative branch remains under the control of congressional committees. The president and departmental secretaries should have authority to provide long-range planning and to set policy priorities assisted by a strong career staff of responsible civil servants. Despite manifold efforts to reform the administrative branch in recent years, the attentive reader may infer that the situation first described by Woodrow Wilson remains resistant to substantial change.

Price suggests some specific institutional im-

provements. He thinks the executive office of the president should be smaller. The multiple complex congressional controls over the executive need to be reduced. The size and influence of congressional and executive office staffs should be diminished, and greater power to formulate general policies delegated to the president and departmental secretaries. However, no reasons are given why Price's suggested changes are any more likely to be adopted than the myriad similar reforms suggested by countless other academic experts. Yet the personal references and reminiscences included in the footnotes add a humane touch to this abstract discussion and present Price's interesting extensive experiences with influential men in high governmental positions.

The Watergate era reforms designed to reduce the power of the "imperial presidency" are replaced here with demands to increase the president's power to formulate public policy and to control the administration. Price returns to the old idea that it is the weakness, not the power, of the president that lies at the center of contemporary problems with the American system of government. Therefore, he claims that we need to restore a greater measure of confidentiality in the preparation of administrative policy and staff papers. The increased role of civil servants in formulating policy needs to be protected from excessive pressures for action by the media. The American and congressional prejudice against delegating power and responsibility for the formulation of policy to the president and civil servants needs to be eliminated.

Price's discussion of "Saints and Scientists" confuses seventeenth-century American puritanism and Calvinism with nineteenth-century southern fundamentalism. The venture into the dense woods of religion and philosophy by this noted student of administration neither illuminates nor enlivens his book. Little evidence is presented to sustain his imaginative hypothesis relating the ideas of dissenting religious groups and modern scientists. The presence of Joseph Priestly and Richard Price in America does not forge any necessary connection between dissenting religious groups and modern scientists. The ideas of John Stuart Mill as expressed in *On Liberty* are closer to contemporary intellectuals' demands for freedom than are the views of dissenting Protestant theologians. No doubt there is a serious theological issue involved in the modern scientist's assertion of a right to freedom from governmental control which deserves a more extensive consideration than it receives here. If the huge sums necessary to sustain research are being provided by public taxes, then the question of the scientist's right to private political views and the right of private universities to be free from governmental

control and accountability may be resolved by the old maxim: He who pays the piper calls the tune.

M. SUSAN POWER

Arkansas State University

Constitutional Law of the Federal System. By C. Herman Pritchett. (Englewood Cliffs, N.J.: Prentice-Hall, 1984. Pp. xiii + 382. \$17.95.)
Constitutional Civil Liberties. By C. Herman Pritchett. (Englewood Cliffs, N.J.: Prentice-Hall, 1984. Pp. x + 406. \$17.95.)

Pritchett is the dean of public law specialists in political science. The books under review are the product of his life's work. Together they are the most informed, insightful, and lucid presentation of American constitutional law I have read.

The subject of each book is the subject of a traditional political science course in constitutional law. *Constitutional Law of the Federal System* begins with a short history of the events leading to the Constitutional Convention and with a brief analysis of the way in which the Constitution allocates political power. In addition to chapters on judicial review, intergovernmental relations, and the organization and jurisdiction of the federal judiciary, Pritchett includes a chapter on the major theories of constitutional interpretation. This is an important discussion, often omitted from constitutional law casebooks. The U.S. Constitution has been amended only 26 times, in part because it is always being reinterpreted by the Supreme Court. An understanding of these theories of interpretation including the intention of the Framers, the original meaning of constitutional language, logical analysis, "experientialism," and "interpretivism" is necessary for an understanding of the history and development of American constitutional law.

Constitutional Civil Liberties, the companion volume to *Constitutional Law of the Federal System*, follows the syllabus for most political science courses in civil rights and liberties. Pritchett examines the proposal and adoption of both the Bill of Rights and the Fourteenth Amendment before turning to the way in which the Court has interpreted these provisions throughout its history. His incisive discussion of the most important First Amendment decisions gives even the casual reader a clear exposition of the Court's treatment of the freedoms of speech, press, association, and religion.

With respect to the application of the due process clauses of the Fifth and Fourteenth Amendments to state and federal criminal prosecutions, Pritchett properly devotes more space to the Fourth Amendment's protection against unrea-

sonable searches and seizures than to Fifth and Sixth Amendment rights. In recent years the Court has been inundated by search and seizure cases, but it has not developed a consistent and predictable standard of review for these cases.

Pritchett devotes two chapters of *Constitutional Civil Liberties* to the equal protection clause of the Fourteenth Amendment. The first of the two chapters concerns racial discrimination; the subject of the second is what Pritchett calls "the new due process—equal protection." This second chapter focuses on how the Court reviews various laws which are challenged as denials of equal protection. These laws classify citizens on the basis of sex, wealth, legitimacy, mental or physical handicaps, and age. This is one of the most interesting areas of constitutional law and one on which Pritchett sheds considerable light.

Economic classifications challenged as violations of equal protection led the Court to define its most permissive equal protection test—the rational basis test. However, when laws classify citizens with respect to such fundamental rights as the right to travel, to receive public education, and to marry or to use contraceptive devices, the Court has employed more restrictive tests.

The most widely used of these tests has been the suspect classification test. Race was the original suspect classification. Whether wealth, sex, alienage, illegitimacy, age, and physical and mental handicaps are also inherently suspect classifications requiring the closest judicial scrutiny is the subject of many of the most important decisions made by the Burger Court. The consensus of the Court seems to be that race is in a class by itself. Classifying citizens according to their race and treating them differently because of their race is almost always a denial of equal protection. How to review laws classifying citizens on the basis of factors such as sex and age is a problem for which the Burger Court has not yet found a cogent solution.

The last chapter of *Constitutional Civil Liberties* deals with elections and the right to vote. Pritchett discusses both constitutional and statutory limitations on state regulation of the franchise. He tells the story of how federal courts have gradually eliminated state restrictions on the right to vote and at the same time have replaced state officials as *de facto* election supervisors.

Pritchett's work in these books demonstrates the truth of the axiom that the only way to understand the Supreme Court is to read and comprehend its opinions. This does not mean that he rejects quantitative methods. In fact, Pritchett pioneered their application to Supreme Court decision making in the 1940s. He has the expertise to evaluate the extent to which public law policymaking can be quantified. His work is required

reading for any political scientist whose constitutional law research is more than an exercise in refining empirical methods.

STEPHEN A. GRAHAM

Indiana Central University

Third Parties in America: Citizen Response to Major Party Failure. By Steven J. Rosenstone, Roy L. Behr, and Edward H. Lazarus. (Princeton, N.J.: Princeton University Press, 1984. Pp. viii + 266. \$25.00, cloth; \$8.95, paper.)

In most courses or texts dealing with U.S. political parties and elections, the subject of third parties is given only cursory treatment. *Third Parties in America* serves to remind everyone of the crucial impact third parties have on the development and performance of the party system. As the authors note in their introduction, "the power of third parties lies in their capacity to affect the content and range of political discourse, and ultimately public policy, by raising issues and options that the two major parties have ignored" (p. 8).

The first half of the book is a discussion of the considerable obstacles that discourage third-party activity. In addition, there is an excellent review of the history of third-party movements during the nineteenth and twentieth centuries. The authors conclude that the "important challengers of the nineteenth century came from relatively stable minor *parties*, whereas those of the twentieth century came from independent candidates" (p. 119). During this century, once the candidates stop running the parties rapidly deteriorate. In this respect, modern third parties seem to be reflecting the more candidate-centered politics that is so prevalent today.

The second half of the book is devoted to a more ambitious task. Using time-series data from the 1840 to 1980 presidential elections and survey data from the 1952 to 1980 elections, Rosenstone, Behr, and Lazarus construct models to explain why both voters and candidates are willing to sustain the considerable costs involved in such activity. They argue that significant third-party activity results from the failure of the two major parties. When the Republicans and Democrats "fail to provide prosperity, responsive policies, and competent, trustworthy leadership" (p. 150), voters are much more inclined to support third parties. Furthermore, if such parties offer "nationally prestigious" candidates, third-party voting is boosted substantially. The authors also find a positive relationship between the age of the party system and third-party voting. As the party system ages, partisan attachments weaken in part because older, more loyal voters are replaced by

newer, more independent ones. Not as firmly anchored by partisan attachments, these new voters are more susceptible to the message of a third party.

The explanation of why candidates are willing to bolt the major parties is somewhat different. As one might expect, third-party efforts are much more likely when either of the major parties have been racked by serious factionalism. Virtually all nationally prestigious third-party candidates first have been rebuffed by their own parties. Because they cannot pursue their goals within the major parties, they make use of third parties. Also, third-party candidates are often running against incumbent presidents with whom they have serious disagreements. These candidates are not so much running to win, but to deny victory to the incumbent (and thereby demonstrate the importance of the issues and constituencies they represent). Another important factor in a candidate's decision is his perception that the major parties are weak and thus vulnerable. Once again, the age of the party system is an important factor in this calculation. For example, the authors argue that the increase in third-party voting during the past 20 years is attributable to increased intraparty factionalism, the failure of the "major parties to realign around more salient issues," and the increasing numbers of post-New Deal voters who have negative evaluations of the party system (pp. 224-225).

This is an excellent book that will almost certainly become the standard in the field. The writing is crisp, and the theories on third-party voting are carefully spelled out and impressively supported. In short, the volume makes a significant contribution to our understanding of the party system in America. Finally, the book is important because it persuasively argues that third-party efforts make an important and positive contribution to American democracy.

JOHN W. EPPERSON

Simpson College

Congress and the Presidency in U.S. Foreign Policymaking: A Study of Interaction and Influence, 1945-1982. By John Rourke. (Boulder, Colo.: Westview Press, 1984. Pp. vii + 441. \$30.00, paper.)

Despite its title, this book focuses more of its analysis on the role of the Congress than of the president in foreign policymaking. Its basic message is that too often Congress has been overlooked as a source of American foreign policy in the postwar years. Although there was increased

congressional activism in the 1970s, the resulting policy influence was not markedly different than that exercised by the Congress in the years immediately after World War II. Furthermore, between these two periods, the Congress played a continuous, if less noticeable, role in foreign policy. At present, Congress seems to be returning to its more normal secondary role in the foreign policy process.

The book is divided into three parts and nine chapters, roughly organized chronologically from the Truman years to the present. The first three chapters focus on the early Truman years and the crucial role of Congress (especially that played by Senator Arthur Vandenberg of Michigan) in the shaping of postwar and cold war policies. The second part begins with Truman's second term and runs through the Nixon administration, but this part also includes three chapters that examine the Congress more directly. The first of these analyzes Congress' influence on policy toward various regions of the world (e.g., Europe, Asia, Middle East); the second on specific issues (e.g., trade policy, foreign assistance, the selection of executive personnel); and the third on "congressional values" in international affairs, treating such items as congressional deference, the role of partisanship, the impact of ethnic groups, and the influence of public opinion, among several others. The last part of the volume, consisting of two chapters, discusses congressional activism in the 1970s and early 1980s and assesses the role of the Congress versus the president in the future.

The technique of analysis in the book is generally to identify various foreign policy decisions and to delve into the interaction process between the executive and legislative branches. Extensive use is made of the manuscript and oral history collections from the various presidential libraries and the private papers of former congressional and executive officials, among others. In addition, of course, much secondary source material is employed: The footnotes are especially extensive and useful, running to 98 pages in all, whereas the bibliography consists of another 21 pages. The result is a volume rich in quotations—some familiar, some less so—from important political actors, and one that organizes in a generally understandable way both familiar and new information on the role of the Congress in foreign policymaking. The most comprehensive analysis, however, is really confined to the early postwar years. As the book gets closer to the present, the analysis becomes more selective. Moreover, even though the title indicates coverage through 1982, there is relatively little on the Reagan administration.

Despite the wealth of material in the volume, the book does have several shortcomings that weaken its overall utility. First, the book attempts

to use the conceptualizations of Abraham Kaplan and Harold D. Lasswell on the notions of "values" and "means" of influence to interpret the process between the Congress and the president. Unfortunately, this approach does not really work very well; instead, it appears to be simply tacked on, rather than integrated into the work. The book would have been better served by relying on a chronological development only. Second, although one comes away from the volume convinced that Congress has not been given enough credit for shaping American foreign policy, the extent of this influence is still not wholly clear. Rarely does Rourke demonstrate that the president did not get the basic elements of his foreign policy, and the author ultimately does endorse Aaron Wildavsky's "two presidencies" argument near the end of the volume. Perhaps a more systematic approach to evaluating the Congress might have provided a firmer answer. Third, the volume needs a careful proofing. It is marred by several key misspellings and factual inaccuracies. Moreover, at one place (p. 236) almost an entire paragraph is repeated. Although these problems are not an insurmountable encumbrance to appreciating the entire volume, too often they distract the reader's attention.

On balance, though, the book provides some good information on the Congress in foreign policymaking, and it could be a useful reference for those interested in congressional-executive relations.

JAMES M. MCCORMICK

Iowa State University

Making Economic Policy in Congress. Edited by Allen Schick. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1983. Pp. xii + 282. \$19.95, cloth; \$10.95, paper.)

Political scientists traditionally tended to study in bits and pieces how Congress made economic policy. An earlier generation of scholars sought to explain behavior in the institutional centers for creation of particular sorts of economic policies. Richard Fenno's *The Power of the Purse* (Little, Brown, 1966) and John Manley's *The Politics of Finance* (Little, Brown, 1970) are examples of this approach. The budget process served as the topical vehicle for more comprehensive assessments of economic policymaking; Aaron Wildavsky's *The Politics of the Budgetary Process* (Little, Brown, 1984) remains unsurpassed among studies of this scale.

The last decade has witnessed far-reaching changes in both economic policy substance and

process in Congress, presenting political scientists with much theoretical and empirical catching-up to do. Fortunately, books rich in empirical description have addressed these changes, such as Larce T. LeLong's *The Fiscal Congress* (Greenwood Press, 1980) and Allen Schick's exhaustive and indispensable *Congress and Money* (Urban Institute, 1980). Schick has now edited a volume of readings that complements our theoretical and empirical understanding of this subject.

All students of Congress should also be students of its economic policies, given the Constitutional and institutional importance of these responsibilities and their impact upon national life. A principal analytical task here entails explaining interrelationships between congressional behavior and a wide range of economic situations such as fiscal policy, income distribution, business regulation, and international trade. Many of the selections in this volume successfully embark on this task.

Catherine Rudder demonstrates that the increasing irrationality of tax policy is related to changes in the personnel and processes of the Ways and Means Committee. John Ferejohn reveals how congressional structure accounts for the diversity and limited effect of income transfer programs. The deleterious policy effects of frequent congressional intervention into administrative regulatory processes are explained by Mark Nadel. Robert Pastor shows how Congress has resisted protectionist legislation over the past 50 years far more frequently than is commonly thought.

The book's most important conceptual contributions are found in an essay by John Ellwood on the budget process and in Schick's concluding essay. Drawing upon earlier work by Theodore Lowi, both authors demonstrate that changing member perceptions of the substance of specific tax and spending decisions have brought about marked changes in congressional fiscal policy behavior. As Schick puts it:

The ability of Congress to define redistributive policies in distributive terms is an important determinant of its capacity to legislate. . . . But Congress has found it increasingly difficult to behave in this manner; hence its legislative output has declined. (p. 260)

Ellwood notes that this resulted because of the new congressional budget procedures and sluggish macroeconomy of the last decade. The legislative success of Reaganomics in 1981, he argues, occurred because it was based upon economic theories that promised an escape for Congress from the contentious redistributive politics of the late seventies. That promise has yet to be realized.

A major lesson from the studies in this volume is that scholars can benefit greatly by studying lawmaking in Congress within the substantive policy context of its operation. A complex and important set of interrelationships between economic conditions and congressional processes is evident here, indicating how the recent growth of interest in political economy in our discipline promises to enrich our understanding of institutional behavior.

This book is essential reading for students of Congress. Throughout, the authors examine not just congressional politics and processes, but also the important interrelations between the perceptions of policy substance and behavior of members. The selections are carefully written and in aggregate contribute a more comprehensive portrait of institutional operations in the context of the economy. Let us hope such good work continues.

STEVEN E. SCHIER

Carleton College

Constitutional Faiths: Felix Frankfurter, Hugo Black, and the Process of Judicial Decision Making. By Mark Silverstein. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 234. \$24.95.)

Biography maintains links between belief, action, and social life that are sometimes ignored in more scientific forms of inquiry. The tradition of judicial biography running from Carl Brent Swisher through Alpheus Mason to J. Woodford Howard has placed the lives of justices like Field, Brandeis, Stone, and Murphy in the context of the institutions and ideology of the law. Recently, there has been a resurgence in biographical work. The new studies are characterized by methodological diversity while (perhaps fortunately) they focus on some of the same individuals. Silverstein's study of Frankfurter and Black looks at justices who have been popular with contemporary biographers. Like the other modern efforts, this is more than a simple life story, and it ranks with the best of the new work.

Although we may not need this book to learn about these two justices, the study is built on good biographical material and the writing is a pleasure. In the tradition of the biographer, it contains original research. New sources chart the intellectual cross currents bearing on how these men viewed judging. For Frankfurter, correspondence with Secretary Stimson (p. 69) shows his early deference to the legislative process. In Black's case, there are notes in the margins of

draft opinions to document the move toward absolutism on First Amendment issues (p. 182). In addition, there is effective and insightful synthesis of secondary sources and public records.

However, the reason for going to the book, and the most exciting basis on which to evaluate it, is Silverstein's promise to build on the political jurisprudence of the last 40 years by examining "how and at what stage" (p. 17) the values and attitudes of a justice operate in the decision-making process. The issue is an important one, and Silverstein has given us some valuable source material. His answer has two facets that are worthy of attention.

One is the suggestion that beyond the familiar notion of "role" is one of "institutional norm" (p. 19) or "process" (p. 211). In the behavioral context, the role variable was treated as an internalized standard of behavior. Although as such a standard role was a necessary corrective to the excesses of behavioralism, it still reflected a limited understanding. Now we are beginning to see, in work like Silverstein's, that norms about judging depend on belief. In the context of an institution, norms are real and consequently more significant for knowing an institution like the Supreme Court than the concept of role had been. Silverstein has deepened our understanding of how confrontations like those between Black and Frankfurter are mediated by institutions.

The second dimension of the book is an explicit interest in the influence of ideas on judicial thought and action. This is the special contribution of the political scientist. Silverstein handles historical theory well, and he has equal facility with contemporary accounts of politics. It is from this background that he interprets the impact of Senator Black's extensive reading of the classics. "From this early reading came the first evidence of the conflict theory that was to dominate his view of the polity" (p. 104). Here, the technical language of social science is used to describe ideas that become the basis for judicial action. Silverstein gives us a glimpse of ideas constituting how Black and Frankfurter saw themselves and their place in the institution. The ideas about democracy and how judges should behave are central to the story of these two very different liberals.

Throughout his book, Silverstein has avoided a focus on outputs or votes in favor of attention to what he calls the "work product" (p. 22). This is an outstanding aspect of the study, and it is essential for understanding the link between process and ideology. Bench memos, briefs, and other internal documents effectively portray the politics of law as battles over interpretations. As a perceptive biography, *Constitutional Faiths* links the interpretations to long-standing beliefs about the world.

Mark Silverstein has captured the strains of constitutional jurisprudence that raise its practitioners above the free play of interests without losing sight of the inevitably political character of this activity.

JOHN BRIGHAM

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Committees in Congress. By Steven S. Smith and Christopher J. Deering. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. xiv + 291. \$10.25, paper.)

Smith and Deering boldly assume a substantial task: to describe and explain the dynamics of congressional committee politics, especially during the past 15 years. Not only has this been an era of change in congressional behavior, but also during the same period political scientists have produced an intimidatingly large literature relevant to congressional committee behavior.

Smith and Deering's model for change in individual committees is simple, but it provides an adequate and comprehensive framework for their analysis. In addition to a data base of interviews with 187 committee members and staff from 1981 to 1983, they have ably drawn upon the rich literature of the discipline and have innovatively gathered interesting data from primary sources, for example, the percentage of bills managed on the floor by chairs of full committees. The data not only provide comparisons across the committees, but also selectively show comparisons for certain years over about a 20-year period. Although the overall pattern is one of decline, trends for committees are far from uniform. Indeed, the pattern of data obtained for the Senate in the 86th Congress (1959 to 1961) would probably surprise most political scientists.

The authors' historical review, in less than 20 pages, of committee institutionalization from the early nineteenth century to the 1960s is an admirable accomplishment. They provide a good review of the committee reforms during the 1970s in another 20. This section crisply sums up the effects of the 1970s' Legislative Reorganization Act, the Budget and Impoundment Act, and the accomplishments associated with the Hanson, Bolling, and Stevenson committees.

However, once Smith and Deering get into the object of their inquiry—to cogently differentiate the patterns of behavior in matters such as committee agendas, the constraints upon the various committees from environmental pressures, member goals in committee activities, and contexts of committee and subcommittee leadership—the immensity of their task becomes evident. To deal

with the House they must comprehend 19 to 22 committees over the period, with as many as 140 subcommittees (94th Congress); at the same time, the Senate had 15 to 18 committees with a peak of 127 subcommittees (93rd Congress). Smith and Deering have difficulty articulating generalizations because there are so many interesting variations among their specific findings. For example, in a discussion of the "interbranch environments" of the committees, four types are distinguished. But even within the types there are obvious differences among similar committees, so a disproportionate share of this discussion describes differences between the House and Senate appropriations committees, both of which are included in a single type.

The authors' proclivity to report details will make the book difficult for students. There are some other deficiencies on that score as well. In an important discussion about jurisdictional fragmentation, the data for a helpful analysis are recorded in the appendixes of the book. However, why the numbers indicate fragmentation is not well explained. Fragmentation is later related to committee staff size, and there (Table 7-1) a footnote suggests that one of the authors' tables concerning fragmentation was deleted in publication.

Smith and Deering are generous in providing data tables, but in certain instances the data bear more discussion than the book provides. For example, they report the number of witnesses appearing before the three types of committees (executive, congressional, and public) for the 91st and 96th Congress. Little inference is made about the substantial increase in the appearances by public witnesses, and whether or to what extent the multiplication of subcommittees has enhanced the access of the public to Congress.

Perhaps the most disappointing aspect of the book is its abrupt ending. After 270 pages of discussion and analysis, Smith and Deering conclude with the timid assessment that "We have found that committees still matter . . . [and] their role in the legislative process is assured." The authors' analysis provides them with data about the persistence of an institutional form, but they choose not to offer substantive judgments about its effectiveness.

JACK R. VAN DER SLIK

Sangamon State University

Louis D. Brandeis: Justice for the People. By Philippa Strum. (Cambridge, Mass.: Harvard University Press, 1984. Pp. xv + 508. \$27.50.)

Philippa Strum's biography of Louis D. Brandeis is the latest in a series of attempts by legal

scholars to chronicle the depth and breadth of Brandeis's remarkable life and career. The task is a formidable one; even the most prominent in public life today pale in comparison to Brandeis. A leader in the Progressive movement, Brandeis was a tireless advocate in support of almost every reform movement of his age including women's suffrage, trade unionism, trust busting, and hours and wages legislation. At the turn of the century, he was one of America's most successful attorneys, earning more than \$2 million before his appointment to the Court in 1916. At the same time, he was known as the "people's attorney," often eschewing fees when advocating causes in the public interest. He was a trusted advisor to Wilson's New Freedom and Roosevelt's New Deal. Although never a religious Jew, he was, nonetheless, the spirit and unifying force behind the Zionist movement in America. During the more than 20 years in which he served on the Supreme Court, he earned a reputation as one of America's greatest jurists. Small wonder that among his friends and admirers he was known as "Isaiah."

Through a detailed study of Brandeis's pre-Court career, Strum attempts to highlight the development of the values and ideas that shaped his life. She succeeds in this task admirably. Brandeis's efforts to reform the life insurance business, to achieve efficiency through scientific management and worker participation, to enforce antitrust laws, to mold the development of the law through an appreciation of the facts of modern life are chronicled in extraordinary detail. In a fascinating section, Strum connects Brandeis's study of ancient Greece with his zeal for Zionism; in Palestine, Brandeis sought to recreate a modern version of the Greek *polis*. This is biography at its best, and our understanding of Brandeis as well as the Progressive era is increased immeasurably.

Sadly, the same cannot be said for the chapters on Brandeis's Court career. One is left with the distinct impression that Strum's admiration for Brandeis precluded a realistic discussion of the contradictions within his jurisprudence. Brandeis never reconciled the detachment demanded by judicial restraint with his zeal for reform and social change. Hence, the typical opinion of Brandeis the jurist was not unlike the famous "Brandeis brief" of Brandeis the advocate; in both instances the emphasis was on an empirical analysis of the merits of challenged legislation. His colleague and friend Justice Holmes was content to conclude that the legislative choice was not precluded by the Constitution. Brandeis assumed a far more activist position in an effort to show the legislation affirmatively sanctioned by the Constitution and necessary in the quest for social justice. In the hands of a judge like Brandeis, this

method inevitably resulted in a progressive decision. But the method is not judicial restraint, and Strum's efforts to portray Brandeis as the apostle of restraint obscure what might have been an important analysis of how ideas and values shape concepts of judicial role.

This objection notwithstanding, *Louis Brandeis: Justice for the People* is well worth reading. Its portrait of the early Brandeis is rich in detail and analysis. Strum has succeeded in tracing the development of many of the values and attitudes that shaped Brandeis's career; in light of his enduring impact on American life and law, that is justification enough for yet another book on Justice Brandeis.

MARK SILVERSTEIN

Boston University

A Review of Reform and Punishment: Essays on Criminal Sentencing. Edited by Michael Tonry and Franklin E. Zimring. (Chicago: University of Chicago Press, 1983. Pp. viii + 210. \$25.00.)

Although it is not easy to determine what part of the criminal justice system is most likely to be in need of reform, criminal sentencing is currently viewed as a part of the system that is in dire need of change. Several factors may explain this observation. Criminal sentencing is complex, controversial, and does not appear to be attaining its goals. It is also a subject of contemporary concern not only in the United States, but also abroad. The editors of this interesting book recognize this important concern and provide an international perspective on the issue.

The editors, relying on a comparative analytical approach, intend this work to be a basic reference for students of issues in criminal sentencing and are successful in that purpose. A sense of urgency for change in the criminal justice system—a radical rethinking of criminal punishment—pervades the book. This is understandable in view of the existence of historical and contemporary inequities in criminal sentencing procedures found in the United States. The authors of the book's essays may differ somewhat in their professional background and style of writing, but they all share a common concern about the controversial phenomenon of criminal sentencing that is an integral part of the criminal justice system.

A Review of Reform and Punishment: Essays on Criminal Sentencing is divided into three main parts. Part 1 focuses on "Lessons from Abroad" and represents a commentary on the Scandinavian and West German sentencing systems compared to that found in the United States. For example, in

the Scandinavian countries, disparities in sentencing seem less common and less serious than that found in the United States; in West Germany, trials and sentencing hearings are unified, whereas in the United States trials and sentencing hearings are kept separate. As Thomas Weigend observes, in the United States, "judges seldom need to give reasons for their decisions. German judges must account for their decisions both orally and in writing" (p. 38). An obvious and important implication suggested in Part 1 is that the European criminal justice system has much to offer its counterpart in the United States—especially in the area of criminal sentencing.

In Part 2, "The American Experience," various factors affecting criminal sentencing in the United States are discussed. An argument is made for legislative guidelines to be used in sentencing, along with appellate judicial review of these guidelines. Franklin E. Zimring correctly suggests that the next generation of lawyers should be in a position to learn from the experiences of criminal sentencing procedures of the past. "But the draftsmen of the next generation of criminal codes must proceed from a perspective that is more worldly, more comprehensive, and more principled" (p. 119).

"Issues for the Future" is the subject of Part 3, which is composed of two final essays that each focus on a contemporary phenomenon characteristic of the criminal justice system. Norval Morris identifies some of the factors involved in sentencing the mentally ill and suggests that certain considerations should affect the type of sentence. The concluding essay by John C. Coffee and Michael Tonry recognizes the difficulty in achieving a consensus relating to the purposes of sentencing. Such a difficulty results in part from the fact that lawyers, judges, and other judicial officials have a vested interest in the status quo. There is a recognition that "a fundamental tension exists between the goals of sentencing equity and procedural fairness that necessitates difficult and value-laden trade-offs" (p. 155).

The various sources used for the essays not only lend credence to the validity of each author's particular view regarding criminal sentencing, but also may serve as a useful guide for those individuals who are seeking valuable bibliographical information about a particular approach to the subject.

A Review of Reform and Punishment has several benefits and should appeal to a wide variety of individuals interested in the criminal justice system. A reader quickly becomes knowledgeable about the importance of criminal sentencing in foreign countries as well as in the United States. In addition, one becomes aware of some similarities and differences characteristic of the criminal

justice system. Such an awareness provides the opportunity not only for reflecting, but also for perhaps a rethinking of one's own value system regarding the sentencing of criminals. Hence, this book may become a valuable source for new ideas that could form the basis for suggestions needed to improve the American criminal justice system.

WILLIAM E. KELLY

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Environmental Policy in the 1980s: Reagan's New Agenda. Edited by Norman J. Vig and Michael E. Kraft. (Washington, D.C.: Congressional Quarterly Press, 1984. Pp. xi + 377. \$11.95, paper.)

Did the election of Ronald Reagan to the presidency in 1980 signal a precipitous decline in the nation's environmental quality? The contributors to this volume stop short of an unequivocal "yes." However, the evidence they amass suggests that the Reagan administration's efforts to reverse the environmental policy enactments of the 1970s certainly possess the potential for environmental degradation. Reagan's environmental policy agenda became clear early on when the transition task force report of the moderate wing was rejected in favor of the Heritage Foundation's ideologically driven recommendations. This set into motion a series of administrative moves (personnel changes, budget cuts, regulation relaxation) intended to change radically federal environmental policy. What Reagan and his closest advisors envisioned was a redesigned environmental policy system in which the private sector and subnational governments assumed expanded roles while that of the national government shrank. The new test of worthiness for environmental programs was to be their effect on the nation's economy. Yet, as the contributors explain, the administration has been forced to retrench its original objectives. Public sentiment, the intense glare of media attention, and a digging in of congressional heels made their pursuit politically risky.

Environmental Policy in the 1980s pulls together the talents of 17 leading environmental scholars to examine the Reagan impact on the environment. The collection opens with Vig and Kraft's theme-setting essay that identifies policy legitimacy and institutional capacity as relevant criteria to guide the evaluation. The volume is organized into four parts, the first of which addresses agenda-setting. The message of the first part is clear: Despite Reagan's victory, there was no voter mandate for environmental policy reversal. The administration widely misperceived

public opinion. In the second part, the focus shifts to institutional behavior. According to the contributors, an aggressive president armed with a negative policy agenda used unilateral administrative tactics to impose his will. Congress, in this struggle, represented a reactive force as it sought to defend environmental protection laws. The courts continued their support of environmental issues, but their traditional deference to administrative expertise meant that several of the administration's deregulation goals were accomplished. Under Reagan's appointees, the primary agency involved, the Environmental Protection Agency, became an agency stripped of its constituency support. Lettie McSpadden Wenner's comment aptly describes the result. "The only litigant left to argue the environmental point of view in some lawsuits between industry and government were private intervenors who appealed cases lost by government" (p. 196).

To document further the relentless attack of the administration, the third section of the book deals with specific policy areas. In both air and water pollution control, for example, the Reagan administration pursued administrative adjustments to long-standing policy. However, in two newer policy areas—acid rain and toxic waste—an administration strategy of regulatory avoidance fell flat. In fact, allegations surrounding the mismanagement of the toxic waste program led to the massive EPA housecleaning in 1983, a major blow to the administration's plans. The persistent threat of a despoiled environment lurks in other policy areas as well. The negative impact of the Reagan administration in energy conservation, in wilderness land management, and in global environmental cooperation will be disquieting even to a marginal environmentalist. "Nonconservationist" appears to be an accurate descriptor of the policy stances taken by administration officials.

The last section of the volume is primarily a recapitulation and summary assessment of Reagan's performance. In a larger sense, it questions the ability of a popular president to recast consensus policy. Despite the rebuff of Reagan's initiatives, the vision of the immediate future is not filled with unbridled optimism. Environmental protection, amid competing budgetary priorities and plagued by scientific uncertainties, faces an arduous challenge.

Criticisms of the book are minor. Only infrequently does the data-rich analysis slip into prescription. Contributors are careful to avoid polemicism, going to lengths to point out aspects of the Reagan plan that may have produced environmental benefits. With 17 contributors, there is some repetition. For example, the virtual demise of the Council on Environmental Quality

is recounted several times as is William L. Ruckelshaus's admission that the administration had misread public opinion regarding environmental protection. Too, the book lacks an index. These shortcomings fade however alongside the volume's significant contribution to the environmental policy literature.

ANN BOWMAN

University of South Carolina

School Desegregation Plans that Work. By Charles Vert Willie. (Westport, Conn.: Greenwood Press, 1984. Pp. xi + 239. \$29.95.)

This book offers an equity-centered approach for measuring school desegregation efficacy. Past the thirtieth anniversary of *Brown v. Board of Education*, desegregation remains a controversial issue with parents, school administrators, politicians, and political scientists debating results of what has and has not been done to create unitary school systems. The debate remains unresolved among political scientists at least partly because of disagreement on what constitutes acceptable evidence. Some political scientists link effectiveness to lack of intergroup conflict and white flight. Charles Vert Willie puts a greater emphasis on examining educational attainment criteria. For Willie, desegregation works if the plan attempts to achieve equity among pluralistic school populations, attain teacher and administrator diversity, and develop ways to enhance the teacher's ability to work with children and a curriculum with multicultural concerns.

Willie's perspective leads him to be more optimistic than most of the researchers who concentrate on conflict and white flight. He argues that many current plans do work in educational equity terms. To illustrate this, he presents a comparative examination of desegregation planning and implementation in Boston, Milwaukee, Seattle, and Atlanta—four cities with very different demographics, politics, community relations, and local power structures, but that are similar in that their desegregation plans increased attainment of Willie's educational equity criteria.

Willie buttresses his argument with descriptive chapters on each city written by key participants in local desegregation efforts. Some of the authors are: David Bennett, deputy superintendent of the Milwaukee Public Schools; Alonzo Crim, superintendent of the Atlanta Public Schools; Robert Dentler, court-appointed expert in the Boston school case; and William Maynard, Seattle's desegregation director. The Boston chapter is particularly interesting because Dentler seeks to overcome popular stereotypes and the assess-

ment of some prior research. Most accounts of Boston concentrate on the negative aspects of Judge Garrity's intervention. Dentler argues instead that the Boston plan "achieved its racial equity goals. It also upgraded facilities, reformed the integrity of the board and administration, and brought excluded minorities into the circle of system concerns" (p. 78).

This revised appraisal of the Boston case continues in a subsequent chapter in which Willie and research assistant Michael Fultz compare Boston's white flight with patterns in Milwaukee, where students have greater school assignment choice. Willie and Fultz argue that if mandatory school assignment causes flight, Boston's loss of white students should have happened at a faster rate and with significantly greater magnitude. Finding few differences in pace or extent of white evacuation, they challenge the conventional wisdom that white flight in Boston occurred because of mandatory student assignment. For Willie, the rallying cry, "No bussing. We don't want another Boston," represents a serious misreading of the situation.

The book's equity perspective makes it important reading for those concerned with educational politics. To understand some parameters of its place in the academic debate on desegregation, one should read it in conjunction with the chapters on defining equality in David L. Kirp's *Just Schools: The Idea of Racial Equality in American Education* (University of California Press, 1982).

HINDY LAUER SCHACHTER

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Monetary Politics. The Federal Reserve and the Politics of Monetary Policy. By John T. Woolley. (New York: Cambridge University Press, 1984. Pp. xii + 280. \$37.50.)

Political scientists have devoted considerable research to analyzing how the pursuit of electoral success has resulted in the manipulation of the economic environment. Although not all scholars concur as to the regularity or periodicity of these politically generated business cycles (or even to their existence), they do agree that as an electoral strategy manipulating the economy makes a great deal of sense. Strategic intent, however, does not ensure performance. *Monetary Politics* is a welcome addition to this literature. Scholars searching for the politically induced business cycle must not only find motive, but also identify opportunity. Woolley's examination of the Federal Reserve Board (FRB) expands our knowledge in both directions.

Much of the existing research on political business cycles focuses on fiscal policy. Not at all a bad choice, considering that it is the structure of budget outlays and receipts that is controlled most directly by the president or Congress. However, one need not be a monetarist to recognize that monetary policy has an important impact upon inflation and the rate of growth of the economy. Yet because of the Federal Reserve Board's statutory independence, too little attention has been paid to monetary aspects of political business cycles.

Into this relative vacuum steps *Monetary Politics*. Woolley examines the Federal Reserve Board as a policymaking institution rather than searching the data for relationships between electoral timing and monetary policy innovations (although he does a bit of this too). This permits Woolley to trace the steps of policy development. The heart of this book examines the relationship between the Federal Reserve and each of the actors or interest groups that might circumscribe the FRB's independence. The pressures upon the Federal Reserve Board to respond to political considerations are intense. Articulate and powerful lobbies argue strenuously on both sides of the unemployment versus inflation issue. Monetary politics describes the Fed's careful balancing act between economic growth and inflation. Does the Fed make its decisions solely on the basis of technical considerations, or does it bow to political persuasion?

In successive chapters Woolley analyzes the relationship between the Fed and the banking community, economists, the president, and Congress. Woolley analyzes both the incentives and the capacity of each major actor to influence the Fed. Is there sufficient unanimity of opinion in the Congress to permit it to direct the Federal Reserve? Do economists have control over the opinions of Board members? How influential is the president in setting FRB policy? Woolley shows that none of these groups are able to control the

actions of the Federal Reserve Board. In a tightly argued chapter, Woolley shows that Congress neither has the inclination nor the purpose to direct monetary policy. The independence of the Board is a functional arrangement that allows individual congressmen to stay afloat in the murky waters of monetary economics while appearing to satisfy constituent demands. The chapter on presidential-Fed relations shows that despite intermittent attempts at jawboning, the president also fails to control monetary policy. Woolley believes the historical similarity between presidential positions and Reserve Board policies reflect a shared consensus on the correct course of action. When no such consensus exists, Woolley views the positions taken by the Federal Reserve Board as institution preserving; the Board assumes policy positions in an attempt to reduce political threats from the outside.

Monetary Politics is a valuable addition to the literature because of its attention to the monetary side of the political business cycle and for its institutional analysis. When balanced against its strengths, the weaknesses of this book are small. I think fault can be found with the type of "class analysis" that views Board members graduating from the same elite schools as holding a set of common interests. Further, what little statistical analysis presented is not always convincing. Evidence of a monetary cycle should probably focus on presidential election years versus nonelection years rather than the odd/even pattern that Woolley uses (p. 128). In addition, as Woolley recognizes, pair-wise analysis of interest groups may not capture actors acting in concert. Finally, I wish the research in this volume had included the Reagan-Volker period. I think the current set of monetary policy issues would have made valuable grist for the Woolley mill.

MICHAEL WOLKOFF

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Comparative and Other Area Studies

Spain: Conditional Democracy. Edited by Christopher Abel and Nissa Torrents. (New York: St. Martin's Press, 1984. Pp. ii + 198. \$24.50.)

Democratic Politics in Spain: Spanish Politics after Franco. Edited by David S. Bell. (New York: St. Martin's Press, 1983. Pp. xiii + 203. \$25.00.)

There has been far too little analysis of Spain's unique transition from authoritarianism to

democracy after the death of Francisco Franco in 1975, and an even more unfortunate paucity of qualitative research on the remarkably stable democracy that has since emerged. Taken together, these two edited volumes are an invaluable addition to English-language scholarship in the latter area. Both books have assembled a collection of articles that will be invaluable for specialists and accessible for all students of comparative politics.

The contributions to the Bell volume emphasize Spain's similarities with West European democracies. Spanish politics are, as Bell argues in his preface, surprisingly un-Latin. The disintegration of the party of the transition, the Union of the Democratic Center, convincingly explained by José Amodia, did not lead to a polarization of Spanish politics as many had feared. Elizabeth Nash's chapter examines the rapid moderation of the Spanish Socialist Workers party, an important factor obstructing such a polarization. Bell's contribution attributes the self-destruction of the Spanish Communist party, another ingredient of the centripetal nature of the party system, to the unresolved contradictions of Eurocommunism and the intransigent authoritarianism of the party leadership. Bruce Young contributes an informative account of the historic elections of October, 1982 that produced Spain's first ever single-party majority government and the young democracy's first alternation of power. What is lacking from the discussion of the major political parties is a chapter on the conservative Popular Alliance, now Spain's second largest party. The Popular Alliance is clearly one of the keys to the future of Spanish democracy, and a party whose democratic credentials have at times been called in question.

The Bell volume also contains two chapters treating areas in which Spain's politics continue to diverge from its West European counterparts. Mike Newton's chapter on the peoples and regions of Spain provides a cogent overview to an extremely complex aspect of the Spanish polity. Pedro Vilanova is, not surprisingly, the most pessimistic of the contributors in his consideration of the Spanish armed forces, which continue to pose the most formidable threat to parliamentary democracy. The three remaining contributions fit into the collection more awkwardly. Peter Holmes provides a straightforward description of relations between Spain and the EEC. Most readers will find the chapter on Spanish social structure, written by José Cazorla Perez and J. Mentabes Pereira, too descriptive and somewhat less useful. The book's major weakness is the inclusion of Pierre Subra de Biesses's piece on constitutional norms and central administration. It is dry, overly technical (much in the legalistic tradition of Spanish political science), and out of place in the volume.

The overall tone of the Abel and Torrents book is less favorable toward Spanish democracy. A number of the contributors question the profundity of the democratization process and point to the regime's inability to control the military, redistribute income, secularize politics, and narrow the gap between political participation of the sexes. The contributions of *Spain: Conditional*

Democracy overlap with those of *Democratic Politics in Spain* in three areas. There is a superb chapter on ethnic nationalism by Salvador Giner and a valuable essay on the Spanish Army by Paul Preston. In addition, Angel Viñas has provided an important analysis of the international dimension of Spanish politics, specifically, relations with the United States and NATO. *Spain: Conditional Democracy* places much less emphasis on party politics and focuses more on social and cultural aspects of Spain's new democracy. Audrey Brassloff provides a competent treatment of the role of the Catholic Church in post-Franco Spain. Nissa Torrents explores the renaissance in cinema and the media since the death of the dictator, and Juan Masoliver documents how the political change has influenced Spanish literature.

A particularly provocative contribution is Monica Threlfall's examination of women and political participation in the new democracy. Her chapter reveals a disturbing absence of women in positions of influence in the governing Socialist party and a less surprising lower voter turnout. There are two general treatments of the transition. Shlomo Ben-Ami's chapter on the legacy of franquism provides some interesting insights into Spain's transition from above, but Juan Antonio Ortega Díaz-Ambrona's highly descriptive synopsis of the Spanish transition does not add as much to the volume. Like its counterpart in the Bell collection, the contribution on the economy and popular movements, written by a team of scholars from the Universidad Central de Barcelona, seems too cursory to be of much use. To its credit, and in contrast to the Bell book, the Abel and Torrents volume includes a bibliography and index.

On the whole, both works are needed additions to a much understudied area of comparative politics. *Democratic Politics in Spain* would be better for classroom use, and is, on the whole, more informative and rigorous. *Spain: Conditional Democracy* treats an interesting series of topics not previously written about and is an excellent complement to the Bell volume. Despite their different emphases, both works underscore the considerable achievements of Spanish democracy, as well as the persistence of obstacles and limits. So far, Spain has managed to avoid the political instability of neighboring Portugal or the paralysis of Italian democracy. All the contributors would likely agree with Vilanova's assertion that Spain is a "weak, not particularly effective democracy, but a democracy nevertheless" (p. 147).

DONALD SHARE

University of Puget Sound

The Jewish Community in British Politics. By Geoffrey Alderman. (New York: Oxford University Press, 1983. Pp. ix + 218. \$34.95.)

The roles of Jews in British politics in the modern era raise a variety of issues deserving of distinct accounts in their own right. These include the part played by British Zionism in influencing the Middle East policies of the major political parties, especially in the wake of the Holocaust and the Suez crisis, and the contribution of both high-born Jewish intelligentsia and poorer immigrant Jews to the development of Labour and the trade unions. However, Geoffrey Alderman's well-crafted and lucid book, although inevitably touching on these and other matters, for the most part offers a more general and introductory guide to Jewish political involvement in Britain. The specific theme that integrates the book is the relationship between the political system and those British voters who are Jewish. The question raised is whether there is any recognizable tendency on the part of Jewish electors to react in a uniform way or as a distinct bloc, or both, to political issues.

Given the long-established and proud declarations by leaders of the Jewish community of the successful assimilation of British Jews in British social and economic life, Alderman clearly raises a sensitive issue. There is a strong tradition among Jewish leaders, from the Marranos of the seventeenth century onwards, of seeking invisibility, which has also extended on occasions to opposing more Jews coming into the country, particularly the pauper Jews from Eastern Europe at the end of the nineteenth century, on the grounds that this could raise anti-Jewish prejudices. It was this striving for assimilation that lay behind the campaigns for emancipation in the last century, although the removal of civil disabilities affecting British-born professing Jews, including the prohibition on a parliamentary career, was not supported by the more orthodox Jews mainly from Europe, who felt that it would lead to a loss of religious identity. Consequently, Alderman's conclusion that a Jewish vote exists and that a uniformity in Jewish electoral behavior can be traced to the last century may well upset parts of the Jewish community.

Alderman persuasively shows how the perception of a recognizable Jewish vote by politicians helped to sustain it. Politicians became and remained susceptible to Jewish claims once they realized that it could profit them, which in turn reinforced a distinctiveness in Jewish electoral behavior. There may be a lesson here for Britain's black population and its leaders, for Alderman's account suggests that it is in no groups' interest to be in the pocket of one party, for it leads to an in-

ability to advance interests by threatening to switch allegiance. The black community's longstanding and overwhelming support for the Labour party may have contributed to the largely feeble Conservative attempts to attract their votes. At various times, however, the Jews in Britain have supported the Liberals, Conservatives, and Labour in large numbers, and all the parties currently have pro-Israeli groups within their organizations.

One of the difficulties in the book in considering the proposition that an identifiable Jewish vote exists lies in disentangling the varying causes. For example, to what extent is the current Conservative allegiance of the Jewish vote the consequence of "non-Jewish" factors such as the increased middle-classness and suburbanization of the Jewish community? Alderman provides preliminary electoral and other data to indicate that "Jewish" issues, particularly the Middle East, have provided at least additional support for the Conservative party above that which could have been expected from socioeconomic status alone. Ironically, an appeal by a leading Jewish Cabinet Minister to Jews to vote Conservative to help contain immigrants had a marked success. Many Conservative MPs hold constituencies that are crucially dependent on Jewish electors who largely have a strong attachment to Israel and expect the member to recognize it. Yet the proposition for a Jewish vote, despite the *prima facie* case made by Alderman, may require more subjective data before its full validation. Unfortunately, Alderman appears to lack a comprehensive set of interview data from Jewish electors that would have provided a fuller understanding of their electoral allegiances and their bases. However, *The Jewish Community in British Politics* is a well-written, thought-provoking, and informative piece of work that provides the basis for further detailed scholarship.

ROGER KING

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The National Question in Yugoslavia: Origins, History, Politics. By Ivo Banac. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 452. \$35.00.)

This book represents a prodigious effort to deal with a complex subject. In Part 1, Banac discusses nation and nationality, lands and identities of peoples inhabiting the Balkans, their historical fates, and factors that shaped their mentalities and national outlooks. Despite some dubious statements, this part of the book is perhaps the

most valuable. In Parts 2, 3, and 4, Banac deals with political parties and movements. In Part 5, he considers the work of Yugoslavia's Provisional Parliament and the Constituent Assembly. In Part 6, there are some interesting reflections on the things that stood in the way of a successful union.

Banac is convinced that the Yugoslavs made their greatest mistake when they adopted a unitary system. For this he holds the Serbs chiefly responsible, although he admits that the "Croat political elite, middle classes, and most intellectuals were . . . committed to Yugoslavist unitarism" (p. 128). What is less clear from his presentation is that those representing Croatia in the Provisional Parliament were politically annihilated in the 1920 elections for the Constituent Assembly by the followers of Stjepan Radic, who refused to take part in constitution-making. Banac does point out that "Radic's private correspondence and public espousal of a Croat republic would seem to belie a supposed commitment to a federated Yugoslavia . . ." (p. 235).

It is clear, however, that the Serbs are Banac's *bête noire*. When he quotes Serbian politicians it is to reflect adversely on Serbs and Serbia, but not the other way round. He is hesitant to recognize such Serbian achievements as parliamentary democracy. Although he says that the matters he discusses "are still subject to widely varying interpretations," he either tends to ignore works that offer a different one from his, or he is selective in what he uses. By quoting Ante Trumbić, a leading Croatian politician, Banac suggests that the Serbs received "a position similar to the primacy of Prussia" (p. 133), but does not say that the Yugoslav constitution, unlike that of Germany, reserved no special position for Serbia. He writes of Serbian predominance in ministerial positions, but does not mention that Croat abstentions from parliament largely made that possible. Banac does demonstrate that the Serbs were fragmented into several parties, and therefore, unlike the Croats, could not speak with one voice.

The book contains a few factual errors (e.g., Ivo Andrić regarded himself a Serb and not a "Croat writer," many of the economic statistics that Banac takes from Rudolf Bičanić have been demonstrated to be seriously flawed, and the standing orders of the Constituent Assembly required an absolute majority for adoption of the constitution and not a "simple plurality").

Although the book supposedly ends with the adoption of the 1921 constitution, there are vexing assertions throughout concerning happenings in the 1920s and 1930s that lack context or evidence. Value-laden expressions (e.g., "Mouse King") tend to detract from the analysis. Ironically, in much of Banac's criticism there is evidence of a good deal of freedom, especially of

the press, in the early years of Yugoslavia's history.

Banac gives proper credit to Stojan Protić for his realistic view of the Croatian problem, and he rightly recognizes that Svetozar Pribičević played the role of a spoiler. The inconsistencies of Yugoslav communists on the nationality question are set forth clearly. And it is pleasing to note Banac's use of Great Serbia and Great Croatia (correct translations) rather than the modifier "Greater," which has come to have a pejorative connotation.

Despite its flaws, *The National Question in Yugoslavia* will be regarded as an important, although not final, word on the subject.

ALEX N. DRAGNICH

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The Islamic Threat to the Soviet State. By Alexandre Bennigsen and Marie Broxup. (New York: St. Martin's Press, 1983. Pp. iii + 170. \$27.50.)

Bennigsen and Broxup have collaborated to produce this brief survey of the relations between the Russians and the Islamic peoples before and after the Bolshevik Revolution. The book focuses primarily on the Central Asian peoples within the Russian/Soviet state, although some attention is given also to the Transcaucasus.

Despite what the title may imply, the book does not probe in any depth possible external threats from the Islamic world. Only one chapter is devoted to Soviet relations with the Muslims outside its borders, and even that chapter emphasizes the interaction of Soviet Muslims with the Islamic world. Although the authors posit that the Middle East has been destabilized since 1978 because of events in Iran and Afghanistan, they pursue the implications of destabilization only insofar as it affects the Islamic population within the USSR. The central focus of the book is the internal threat that Muslim loyalties pose to the ideological unity of the USSR. Since 1917, the Soviet Union has attempted to create the new "Soviet man" in a homogeneous Soviet culture. The Central Asian peoples with their strong Islamic heritage present a major obstacle to Sovietization.

The relationship between Russians and the Islamic peoples within Russia goes back to the ninth century and was intensified during the Mongol period in Russia. According to the authors, the Soviet image of the Mongol period remains a negative one, a time in which virtually no benefits accrued to Russia from their relationship with the Mongol hordes. Although oversimplified, over the centuries this image has condi-

tioned Russia's attitude toward Central Asia. Because of their historical perceptions, the Russians felt inferior to the Central Asian peoples whom they conquered (p. 8). Although on the one hand, they have regarded the Central Asians as backward people whom they must civilize, they have not forgotten that the ancestors of those very barbarians once ruled Russia. Conversely, the native Muslims do not feel they are inferior to the Russians. According to the authors, the Russian hatred toward the Muslims "makes the cultural or biological symbiosis between . . . [them] . . . a hopeless dream" (p. 9).

Bennigsen and Broxup outline the diverse approaches the Soviet state has taken to solve their Islamic problem, ranging from elimination to integration of the various Muslim peoples. They also chronicle the religious loyalties of the present-day Islamic population to organized Islam and informal Muslim groups. The authors conclude that the goal of nineteenth-century Islamic reformers, a "modern Muslim secular society" (p. 149), is near fruition within the USSR.

Although their arguments are well defended, the reader may speculate whether the authors overestimate the intensity of the Islamic loyalties within Central Asia and underestimate the efficacy of more than 60 years of Soviet influence. Central Asia has, to some degree, resisted the process of Sovietization and the Europeanization that it implies. Bennigsen and Broxup posit two major problems that will arise by the end of the century as a result of changing demographic patterns in the USSR: the "nativisation of the borderlands" and larger number of Central Asians in the armed forces, the so-called "yellowing of the Red Army" (pp. 131-135). The army and the borderlands will be populated by peoples less than totally loyal to the Soviet regime. Bennigsen and Broxup project that the Muslim nationalities may total as much as 25% of the overall population of the USSR by the year 2000 (p. 130). Will this be a serious problem for the USSR? Soviet sources tend to dismiss concerns about the rapid population growth in Central Asia, whereas various Western sources have suggested that the situation in Central Asia, combined with the stagnation of population growth in the European areas of the USSR, will pose significant problems in the future. Are Bennigsen and other Western scholars correct in assessing the probable impact of these trends?

This brief work is tightly packed with information of interest to Sovietologists and Islamicists. The advanced student will find this a valuable reference source. The book's scholarly value is only slightly diminished by the authors' tendency to skip back and forth through the centuries. Although strict observance of the chronology of

events might be ill-advised in describing the complexity of Russian-Islamic relations within the USSR, greater attention to the sequence of historical events would enhance the readability of this work for the nonspecialist. The glossary can help the reader decipher the numerous Islamic terms introduced in the text, and the bibliography will assist those who wish to read further on this topic.

NORMA C. NOONAN

Augsburg College

Democracy and Elections: Electoral Systems and Their Political Consequences. Edited by Vernon Bogdanor and David Butler. (New York: Cambridge University Press, 1983. Pp. ix + 267. \$39.50.)

Having written about electoral reform in Britain in *The People and the Party System* (Cambridge University Press, 1981), Vernon Bogdanor now joins with David Butler to present a volume dealing with the effect of electoral systems in EEC countries and Japan. Based on a 1982 Nuffield College seminar, the 13 original essays display unusual coherence. Narrowly equating democracy and elections, the authors ask if and how variations in methods for translating votes into seats in legislatures affect the character of governments chosen. Using history, they emphasize the importance of political choice and social context in the structuring of electoral systems and, in so doing, take an important step beyond Maurice Duverger, Seymour Martin Lipset, and Stein Rokkan.

Bogdanor identifies three methods for converting votes into seats: plurality, majority, and proportional systems. The plurality system, linked to territorial representation, provides stable and effective government while producing disproportional representation. Majority systems eliminate the possibility of a candidate winning on a minority vote. Proportional systems (PR), like the Irish single transferable vote (STV) and the continental list system, provide more accurate representation of preferences. Richard Rose points out that the differences between the methods is a matter of degree, while emphasizing that politicians' choices about constitutions also determine representation levels, the character of the legislative assemblies, the eligibility of voters, and the conduct of campaigning.

Surveying variants of the British plurality model, David Butler argues that the effect of the first-past-the-post system depends on circumstances. Even so, plurality systems usually foster two-party systems, penalize minor parties, and encourage moderation. According to David Goldey

and Philip Williams, the effect of regime crises and modernization in France since World War II resulted in a bipolar party system that has enabled the second ballot system, which had contributed to instability during the Third Republic, to produce stable governments under the Fifth. Peter Pulzer argues that the post-World War II German electoral system was less important to the development of stable government than the emergence of a bipolar party alignment and coalition politics.

Turning to PR systems, Christopher Seton-Watson argues that Italy's complex list rules, while perpetuating a multiparty system, unstable government coalitions, the dominance of party, and factionalism, promote a search for consensus among deep ideological and social divisions. Bo Sarlvik contends that PR systems in Scandinavia were an adaptation to the requirements of multiparty systems that achieved stability through the dominance of Social Democratic parties until the 1970s. Dick Leonard's study of the Benelux countries shows that PR systems with more members elected from each district produce more proportional results and more instability. While Paul McKee establishes that Ireland's STV system is popular and effective, Richard Clogg adduces that Greece's "reinforced" PR system favors the Right. As J.A.A. Stockwin demonstrates, Japan's multimember constituencies without a transferable vote reinforce its cultural conservatism, fragmenting the opposition and fostering local and personal factionalism. Finally, Michael Steed discusses direct elections to the European Parliament.

The volume's appreciation of the complexity of political and social context demonstrates the fallacy of Duverger's claim that plurality systems foster two-party competition and stability, whereas proportional representation produces fragmentation and instability; its emphasis on political choice adds to Lipset and Rokkan's use of political archaeology to explain party attachments. However, its use of the notion of democracy is surprisingly uninformed by democratic theory, which focuses on elite competition (Joseph Schumpeter and Robert Dahl) or links participation to popular influence (Rousseau and J.S. Mill). To identify electoral systems with democracy and then to make them the object of political choice simply ignores how they can transmit or contain electors' interests. Because democracy is obviously more than a ritual serving elite's interests in stability, the book would benefit from considering how electoral systems contribute to interest representation and leadership accountability.

JOEL D. WOLFE

Georgetown University

The Western University on Trial. Edited by John W. Chapman. (Berkeley: University of California Press, 1983. Pp. xiii + 238. \$24.50.)

The theme of this important volume is that the Western university has undergone changes during the past two decades that are inimical to its greater purpose, the pursuit of truth.

The book is based on papers presented by scholars in 1981 at the Third International Conference of the Council on the Future of the University held in Lisbon. The authors largely concentrated on experiences in their own countries, but their conclusions are nearly indistinguishable. Virtually all recognize that student protests during the 1960s about civil rights, free speech, and the Vietnam War became the genesis of student participation in the running of universities and the ceding of power by professors and administrators. An ethos of egalitarianism and democratization challenged the traditional hierarchy of power.

In an impassioned critique of American universities, Allan Bloom associates the lowering of standards with "the three cardinal sins of the egalitarian creed—racism, sexism, and elitism." Universities have sought expiation through grade inflation and affirmative action but, says Bloom, "no one who is thinking about such things can be thinking about education and scholarship" (pp. 157-158).

Descriptions of conditions in Europe are worse. Peter Kielmansegg observes that in countries where the government dominates the system of higher education, parliaments have been able to "turn the universities upside down" (p. 47). John Passmore points out that in "German mass universities there is no mechanism for singling out the brightest and best students" (p. 75).

Even more dismaying is Arend Lijphart's essay about university democracy in the Netherlands. The University Governance Reorganization Act of 1970 abolished exclusive control by professors over teaching and research. The act mandated that such activities be determined by councils composed of students, nonacademic staff, and representatives of society as well as faculty. This has led to politicization of the university and a decline of academic standards. Courses are frequently given "an ideological (usually Marxist) slant," while students act like trade unionists "fighting for less work, fewer hours, and easier advancement" (pp. 220-222).

Worrisome as the descriptions are, some appear dated, and related propositions are inadequately explored. In recent years, in the United States at least, competitive universities have reinstituted core curriculum requirements, and students have taken academic responsibilities seriously.

Although several authors yearn for a competitive system based exclusively on merit, there is little discussion about what the term means or how it is to be assessed. Grades and test scores seem to be the authors' favored indicators, but unquantifiable criteria like creativity, initiative, or perseverance are virtually ignored. Moreover, although several authors condemn affirmative action as incompatible with a merit system, no consideration is given to the possibility that a student's education is enhanced when the backgrounds of his peers are ethnically and regionally diverse.

Another unexamined contention concerns the propriety of regulating scientific activities. Kiemansegg labels such restraint as unjustified interference with "the free pursuit of truth," and Martin Bulmer writes similarly about institutional boards that review research on human subjects (pp. 52, 115). Yet neither author discusses the public's right to protection from hazardous scientific activities, and neither notes the past abuses of human experimental subjects that prompted the need for oversight. Although such imbalances are unfortunate, they do not impair the book's principal and compelling argument—that a proper university cannot be a participatory democracy.

Many contributors are pessimistic. They lament that the trends they deplore are unlikely to be reversed, especially in the European universities. They may not have discerned that in some countries changes were already underway at the time of the conference. "In the United States," Robin Winks observed then, "the push, in education as in politics, has been leftward" (p. 185). That was, of course, before the Reagan years.

LEONARD A. COLE

Ridgewood, New Jersey

Theories of Development: Mode of Production or Dependency? Edited by Ronald H. Chilcote and Dale L. Johnson. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 256. \$25.00, cloth; \$12.50, paper.)

Political Development Theory. By Richard A. Higgott. (New York: St. Martin's Press, 1983. Pp. xiii + 124. \$18.95.)

Both of these volumes are primarily directed to specialists in the field of development studies, and each in its own way makes an important contribution. They nevertheless are two quite different kinds of works, and thus will attract substantially different audiences.

Political Development Theory is basically a historiographic reconstruction of the debates in both establishment political science of develop-

ment literature and radical development of underdevelopment literature over the last 20 years. Higgott argues that although the political science of development approach has moved away from the grand theory of its behaviorist phase to a more specifically focused policy science orientation in the current postbehaviorist phase, both share the same assumptions about the nature of political development and thus lead to the same kinds of normative prescriptions. For Higgott, the pessimism about the prospect of development in the Third World that gripped the field of political science in the 1970s and led to perspectives that focused almost exclusively on the maintenance of order and ignored the question of imperialism is still evidenced in the current public policy approach to third-world development. He attributes this to the political science of development's need to maintain its professional image by adopting "the predominant trends within the mainstream of the political science discipline" (p. 43).

In much the same way that he chronicles the evolution of the political science of development approach, Higgott recounts the debates within the development of underdevelopment approach that have led from dependency theory's dominance in the early 1970s to the current mode of production perspective in the 1980s. Here, Higgott's efforts pale in comparison to similar and more comprehensive works like Albert Szymanski's *Logic of Imperialism* (Praeger Publishers, 1981). He nevertheless provides a fairly accurate account of how the more Marxist productionist mode of production thesis arose out of leftist critiques of circulationist dependency theory. However, while chiding dependency theory for ignoring classes and class formation within the context of dependency and underdevelopment, Higgott completely ignores the seminal effort in this regard of Fernando Henrique Cardoso and Enzo Faletto in their *Dependency and Development in Latin America*, first published in Spanish in 1969, long before the mode of production thesis appeared on the scene.

Perhaps the most interesting section of the book is chapter 4, in which Higgott argues that because of these recent revisions in political science of development and radical development theories—policy science and mode of production—they now share a common ground. Both, he argues, share a focus on the micro-political or economic level, and both see the importance of the state in the development process. If both can only put aside their normative assumptions and prescriptions, Higgott feels that a common research program may be possible.

This would certainly be a triumph for apolitical political science but would in effect lead to the sterility of both approaches. What

motivates each approach and guides its direction are just these normative assumptions and prescriptions—the political science of development and policy science approaches are inherently conservative and antirevolutionary, whereas the dependency (regardless of what some on the Left might say) and mode of production approaches are inherently radical and revolutionary. One may question whether or not each meets its mark, but there is no question as to their intent.

Theories of Development: Mode of Production or Dependency? clearly raises the dependency approach from the grave in which Higgott so neatly buried it. The volume is a collection of papers given at a 1979 Congress of Americanists panel that debated the issue of dependency versus mode of production theory. On balance, what clearly stands out from these chapters is that although some few still reject one or the other approach for ideological reasons, the trend is for dialogue and reconciliation in an effort to move on to substantially more meaningful analysis of development in the Third World.

Chilcote contributes an excellent introductory overview of the various essays that is followed by two chapters, one by Aijaz Ahmad and the other by Carlos Johnson, that are basically polemics that go over old ground in the dispute between these two approaches and offer nothing new to the initiated. The second section of the book contains two pathbreaking essays by Anibal Quijano on Peru and Norma Stoltz Chinchilla on Guatemala that are efforts at going beyond the theoretical debate. Quijano's essay more closely follows the *dependencia* formulation of imperialist penetration of Latin America but overcomes its narrowness by arguing that an articulation of monopoly capitalist (of external origin) and pre-capitalist (of internal origin) relations of production have created a situation of semicolonial accumulation in Peru. Chinchilla's contribution on Guatemala is somewhat more searching. Although she ultimately prefers the mode of production approach, Chinchilla never loses sight of the dialectic between the internal and external determinants of underdevelopment and class formation. What is most welcome in her essay is the systematic way she evaluates each approach—modernization, evolutionary Marxist, dependency, and mode of production—with regard to their adequacy in explaining the Guatemalan reality. She concludes by arguing that although the Guatemalan social formation was the result of the Spanish conquest, its subsequent development and transformation can be understood primarily as a result of the internal processes of class formation, accumulation, and politics (the state). The third section of the book includes papers by Andre Gunder Frank, Henry Veltmeyer, and Dale

Johnson. Frank's essay is a reworking of his recent works in which he further collapses every form of production into capitalism—now even socialism, because it has been "relinking" (trading) with the capitalist world, is defined as capitalist. Veltmeyer's contribution deals specifically with the problem of analyzing class formation in the Third World. He concludes that the processes of class formation and capital accumulation in "peripheral" societies are fundamentally different from those in the advanced capitalist countries. Dale Johnson adds an excellent conclusion that argues that the two approaches be joined in a two-level approach—one that addresses the question of the international accumulation of capital and class formation (macro-level) and internal class formation and relations of production (micro-level).

DOUGLAS FRIEDMAN

The College of Charleston

Khrushchev's Party Reform: Coalition Building and Institutional Innovation. By Barbara Ann Chotiner. (Westport, Conn.: Greenwood Press, 1984. Pp. x + 313. \$35.00.)

The Soviet Party-State: The Politics of Ideocratic Despotism. By Carl A. Linden. (New York: Praeger Publishers, 1983. Pp. xii + 174. \$24.95.)

The Khrushchev era is often regarded as a transitional period between the horrors of Stalinism and the conservatism and status quo orientation of the Brezhnev years. Yet it would be quite incorrect to evaluate Khrushchev's influence upon Soviet history solely in terms of personal ambition, for he was an innovator in several respects. First, via the political and theoretical demotion of Stalin at the Twentieth and Twenty-Second Congresses of the Communist Party of the Soviet Union (CPSU), Khrushchev was able to legitimize his own authority and thus prepare the way for new approaches. Second, Khrushchev sought to advance the power and influence of the Party and its apparatus, which had stagnated under Stalin. Third, Khrushchev's theory of the nature of the Party was his primary contribution to ideology; specifically, that the Party had already become a party or vanguard of the whole people, and no longer a party or vanguard of the ruling class, the proletariat. Last, Khrushchev reorganized the Party in November, 1962 into two separate hierarchies, one charged with practical economic leadership in industry and the other in the agricultural sector.

Khrushchev's Party Reform is a study of Soviet policymaking that examines the 1962 bifurcation

of the Soviet Community party from its earliest origins through its implementation and eventual repeal following Khrushchev's removal from power. Effectively utilizing the model of policy-making developed by Zbigniew Brzezinski and Samuel P. Huntington in their *Political Power: USA/USSR* (Viking Press, 1964), which suggests the existence of four decisional phases—initiation, persuasion, decision, and execution—Barbara Ann Chotiner expands this background for policy initiation by offering a fifth phase in the decisional process, a germination stage. By incorporating relevant features of coalition theory, outlined in William Riker's *Theory of Political Coalitions* (Yale University Press, 1962), and the ideational scheme, elaborated in Homer Barnett's *Innovation: The Basis of Cultural Change* (McGraw-Hill, 1953), which looks at the genesis of innovations, Chotiner's work offers several analytical advantages and a superior methodological thoroughness.

Comprised of nine chapters, Chotiner's book opens with the earliest indicators of Khrushchev's views towards Party work and the institutional debates that would precede any substantive organizational change. Indeed, Chotiner's evaluation of the innovative characteristics of the Party's restructuring scheme, and the politics surrounding the approval for discussing the bifurcation recommendations outside the Presidium during the initiation phase, is among the best on these topics to be found in Western analyses. Having received Presidium acquiescence in discussions of his party bifurcation proposal, according to Chotiner, Khrushchev could move more vigorously to persuade his associates in the ruling body and lower-echelon officials in the Party and government hierarchies (p. 111).

Regarding the overall effectiveness of the bifurcation, Chotiner notes the following:

Since many narratives about party aid presented the details of intervention as exemplifying correct production leadership after the bifurcation, the accounts furnish further proof that many *partkoms* concretized their style of work in a manner acceptable to the central CPSU bodies. Furthermore, summations of the local party organs' activity during 1963 and 1964 in the five categories of industrial and agricultural management have indicated the CPSU agencies' increased tendency to help economic leaders implement decisions. The existence of such a trend is another indicator that party apparatchiks' administrative methods became more operative after the November 1962 Plenum. Indeed, the division of the Community Party apparatus into industrial and agricultural components did seem to be accompanied by noteworthy changes in the new party organs' style of work. (p. 265)

In the final analysis, according to Chotiner, as political hierarchs seek to maintain the CPSU's influence, preeminence, and centrality in Soviet society, their ability to build coalitions behind various conceptions of the Party's role and character will depend upon the ability to place their ideas in the context of an exchange of policy promises, patronage, and perhaps even threats (p. 289).

The Soviet Party-State: The Politics of Ideocratic Despotism is, as its title implies, a work that focuses on the ideological bases of the Soviet party-state, without, however, losing sight of the barrier that the Marxian ideology—reinforced by its Leninist content—places before the Soviet leadership and the practically oriented, nonideological needs of Soviet society.

The term *ideocratic*, coined by Berdyaev to identify the Soviet regime, is, according to Linden, peculiarly apt as the classification of a regime that rests its claim to rule on an explicit and totalist ideology. The term *ideocracy* is suggestive of a secular analogue to theocracy, with its close tying of ideas as dogmas and power (p. xii). According to Linden, who is quite correct in his analysis, the structure of dictatorial power that arises out of Leninized Marxism stands out in bold relief, especially when looked at in juxtaposition to the actual role that Marxist-Leninist ideology plays within the Soviet party-state:

Is it the animating source pervading its activity or is it something of an elaborate pretext for the actual operation and self-perpetuation of the system of despotic power? Is it, in brief, its rationale or rationalization? The question already indicates the two broad schools of thought that emerge out of the long and sometimes exacerbated debate among students of Soviet and communist party-states around the nexus of ideology and power. Not surprisingly, the one identifies elements of connection, the other disconnection between the theory and the practice, the ideology and the reality of the party-state system. (pp. 32-33)

Comprised of six full chapters, Linden's book returns to the primary ideological source of the Soviet party-state to the original tenets of Marxism itself, and examines, among other things, the source of Soviet despotism—Russia or Marxism?—the choice of revolution, and the nucleus of the communist dictatorship, a nucleus of knower-intellectuals, the "communists" of the *Manifesto* (p. 23). Linden's chapters on "Marxism-Leninism and Ideocratic Despotism" and "The Ideocratic Party-State and Faction" are characteristic of the book's scholarship and commitment to analytical detail. Indeed, despite its official dedication to monolithism and its projec-

tion of an image of internal unity, the long history of internal cleavage within the rulership makes evident the fact that the Soviet party-state is not by any means a seamless entity (p. x). The last two chapters are concerned with the contemporary party-state, and examine the sharp counterpoint that developed between the Khrushchev and Brezhnev eras.

Linden concludes on the following note:

The ideology that rules the rulers suffers from a critical defect. It lacks any practical wisdom or guidance on how rulers can go about extending political power beyond themselves with a hope of success and without fear of losing it all themselves. In its utopian vision of an ultimate liberation of man from domination, it speaks not of the distribution of political power but its radical disappearance. . . . The question then arises whether a ruler or rulership of the party-state can emerge capable of correcting the practical defect in its creed and if so can it be done without a basic change in the regime of the party-state? (pp. 159-160)

Linden's *The Soviet Party-State* and Chotiner's *Khrushchev's Party Reform* are works which will appeal to students of Soviet politics, comparative communism, and communist ideology.

JOHN M. CARFORA

Dartmouth College

The National Question in Marxist-Leninist Theory and Strategy. By Walker Connor. (Princeton, N.J.: Princeton University Press, 1984. Pp. xvii + 614. \$47.50, cloth; \$14.50, paper.)

Most works on nationalism in communist systems tend to focus either on the resistance of national minorities to the regime's assimilationist policies or on the expression of a nationalist outlook on the part of a ruling party, particularly in its relations with the Soviet Union or with neighboring states. Walker Connor deals with both of these questions, but places them within the broader context of a comparative analysis of how Marxist-Leninist theory regarding nations, nationalities, and nationalism has been applied in practice by various communist parties in their efforts to gain and to maintain power in multinational states. To do this, he has closely examined the variations in the nationality policies of the communist parties of the USSR, China, Yugoslavia, Vietnam and, to a lesser extent, those of Czechoslovakia, Rumania, and other nations.

After first looking at the basic tenets of classical Marxism on the question, Connor concentrates

on the "strategic Marxist" approach developed by Lenin in order both to combat the force of nationalism and to manipulate it on behalf of the international movement. Lenin's solution was the formula which called for national self-determination for ethnic minorities with the right to secession from a federation which would provide them with the forms of national autonomy. This was intended to be a dialectical strategy designed to mollify nationalist sensitivities by allowing the full flowering of national cultures while the centralized and internationalist Communist party organization maintained control of the content and provided the basis for the ultimate elimination of nationalist feelings through an eventual merging of nations.

One major section of the book is devoted to tracing how Lenin's formula in some form was utilized by each of the parties in their efforts to gain power. Using rich historical detail, Connor indicates how, despite the difficulties in adapting it to each party's particular circumstances, the formula generally did prove to be a useful temporary strategem for achieving their aims.

The other major section deals with how the Leninist formula was adjusted or warped to the needs of various parties after they had achieved power. In those cases where the forms of national autonomy weren't completely abandoned, as they were in Vietnam, Connor shows that all the ruling parties, including Lenin's, have felt the need to adopt policies designed to provide a considerable hedge against any real expression of minority nationalism. Special attention is given to the implementation of policies aimed at promoting the language of the dominant nationality, controlling the recruitment and purging of minority personnel, and the gerrymandering of nationality areas to reduce even the forms of autonomy. Connor carefully traces the changes in intensity of application of these assimilationist policies over time in each country.

In arguing that the "Leninist solution" has either not been given a fair chance or has failed as a method for gradually eliminating nationalism within the multinational states, Connor provides a very insightful discussion of several contradictions in Lenin's analysis of nationalism which have made this failure virtually inevitable. Among these are Lenin's failure to recognize that national forms breed nationalist content and his failure fully to appreciate the power of the nationalism of the dominant nationality. It is the latter that has subverted the internationalism of the vanguard parties which Lenin believed would be the guarantors of the success of his policy.

There are several problems with the book. Although it is a thoroughly documented study replete with extensive explanatory footnotes,

Connor's attempt to encompass so many countries has meant that he is compelled to rely overwhelmingly on English-language materials. In addition, the arrangement of his analysis leads to a considerable amount of repetitiousness as he re-examines the policies of each party from various angles and at various stages of their development. Nevertheless, this is clearly an outstanding work of comparative analysis and a major contribution to the understanding of one of the most serious problems confronting the communist-ruled states.

DAVID L. WILLIAMS

Ohio University

Conflict in Northwest Africa: The Western Sahara Dispute. By John Damis. (Stanford, Calif.: Hoover Institution Press, 1983. Pp. 214. \$19.95.)

John Damis's book constitutes a valuable contribution to the growing literature on the Western Sahara. It may come as a surprise that such a "God forsaken scorching desert tract half the size of France with little water and less people" (p. 1) commands so much attention, but the accidents of history have made the Western Sahara a major bone of contention. The former Spanish colony is a major battleground between different nationalisms (Greater Moroccan, Greater Mauritanian, and Saharwi), between Morocco and Algeria who compete for hegemony in the Maghrib, between radical and conservative states in the Arab world and Black Africa, and between East and West. John Damis's book is a solid piece of work on the historical, legal, and ethnic dimensions of the conflict; on the rapid developments in the 1960s and 1970s; and on the shifting positions of the major actors: Morocco, Algeria, Mauritania, the political and military organizations of the Saharwi, Libya, Spain, France, the United States, the USSR, the OAU, and the UN.

Damis's book stands out for two reasons: Contrary to other books on the subject, his analysis is not one sidedly anti-Moroccan, and his balanced nonpartisan approach leads him to offer ideas for a resolution of the conflict, ideas which are realistic although complex.

The Moroccan case is well presented. Damis demonstrates the historical connection between Morocco and the Western Sahara from the times of the Almoravid dynasty to the present. He analyzes the ethnic ties between Southern Morocco and the major tribes of the Western Sahara and explains why it is difficult for Moroccans to understand why it was legitimate to absorb the Spanish protectorates in Northern and Southern Morocco, Tangier, and Ifni without a

referendum, and why the same procedure is illegitimate for the Western Sahara. He shows that because Moroccans view the Western Sahara as an integral part of historical Greater Morocco, they refuse to apply the principle of national self-determination within colonial boundaries in this case. Damis also emphasizes that the conflict is a truly national struggle and not one between reactionaries and progressives, because both the Moroccan nationalists and socialists (and even the communists) support the Moroccan claim to the Western Sahara as much as the king and the conservative forces. On the other hand, Damis does not dispute the existence of widespread anti-Moroccan sentiments among the Saharwi, the growth of a new Saharwi national identity, or the legitimacy of the demand for national self-determination.

Damis's compromise proposals are the most interesting part of the book. He feels that the total victory of one party and total defeat of the other should be avoided for the sake of peace and security. He looks for formulas that will, at least partially, satisfy all the parties concerned: Moroccans, Saharwi, Algerians, and Mauriticians. Damis spells out three formulas for the partition of the Western Sahara between Moroccans and Saharwi and border changes that will assure Algeria a secure and recognized boundary and Mauritania more territorial depth along the vital railway from Zouirat to Nouadhibou. The twentieth-century experience in the Middle East, the Malayan peninsula, India, and Ireland has demonstrated that partition is often the only pragmatic and humane alternative to an apocalyptic all-or-nothing approach. John Damis should be congratulated for his deviation from scholarly analysis and for his readiness to come forward with realistic compromise formulas.

BENYAMIN NEUBERGER

University of Pennsylvania

Terrorismo E Violenza Politica Tre casi a confronto: Stati Uniti, Germania e Giappone. Edited by Donatella della Porta and Gianfranco Pasquino. (Bologna: Societa editrice il Mulino, 1983. Pp. v + 263. L. 15.000, paper.)

On August 2, 1980 neofascists detonated a bomb in the waiting room of the railroad station in Bologna. The explosion killed more than 80 people and left several hundred injured. It was the worst single terrorist incident in the history of republican Italy. In the aftermath of the event, the regional government of Emilia-Romagna, which is headquartered in Bologna, agreed to sponsor a program of research aimed at understanding the role of ter-

rorism in contemporary Italian political life. The Istituto Cattaneo was given the responsibility for carrying out this project.

Terrorismo E Violenza Politica is the first of several volumes to be derived from the research. The purpose of this initial book is to establish a comparative context within which the recent Italian experience may be understood. To this end, the editors brought together scholars from the United States, West Germany, and Japan (Ted Gurr, Irving Fetscher, and Hiroshi Kawahara) so that they could review, for an Italian audience, the experiences of terrorism in their countries. Their contributions, along with a chapter by Charles Tilly on the history of collective action in England and France, constitute the bulk of this book. The case studies are introduced by Donatella della Porta's review of the major themes in terrorism research and concluded with Gianfranco Pasquino's assessment of their relevance to the Italian situation.

The choice of cases selected for comparison with the Italian reflects a most similar systems orientation. Like Italy, the other nations exhibit advanced industrialized economies and are ruled by democratic political regimes. Again, like Italy, they are nations where causes of a separatist-nationalist type have not been the major stimuli of terrorist activity. On the other hand, all four nations have histories of right-wing violence and were sites for the mobilization of left-wing protest movements during the 1960s.

Since the editors' intent is to use the American, German, and Japanese cases to shed light on the Italian, it seems appropriate for the book to be reviewed on these terms. What distinguishes terrorism in Italy from that found in the other countries? Pasquino specifies two features. First, although it is true that terrorism of a fascist, vigilante, or right-wing character has been present in the other countries, as the three case studies make clear, Italy stands out by virtue of the intensity of neofascist violence as well as by its covert encouragement by elements within the state apparatus. Illustratively, in none of the other countries was there a serious possibility that these elements would try to exploit events to initiate a rightist coup d'état as was true in the Italy of the early 1970s. Second, although all four countries experienced significant waves of terrorism beginning in the late 1960s and continuing into the 1970s, Italy's did not crest until the present decade. Left- and right-wing terrorist groups in Italy showed a capacity to endure, reorganize, and recruit new members that their counterparts in the United States, West Germany, and Japan did not display.

If these are the presenting symptoms, what are their causes? Presumably this question will be ad-

dressed thoroughly in the other volumes in this series. Pasquino's preliminary judgment has to do with the interaction between the formation of mass movements demanding sweeping change and willing to take to the streets to achieve it, and a blocked, change-resistant political system. Yet he also recognizes that these ingredients were not totally missing in the other countries.

A final point worth calling to the reader's attention is that conspicuously missing from the accounts of terrorism in the three country studies as well as in the conclusion is any attempt to ascribe common personality attributes to the terrorists as a way of explaining their behavior. The analyses are historical, social, and political, not psychological. The tone is set in the introduction by della Porta, who notes that attempts to stipulate a terrorist personality fail because available evidence does not indicate signs of abnormality. Further, she argues that efforts to attach psychological labels to terrorists serves, deliberately or otherwise, a political purpose. They divert attention from the social circumstances in which terrorism arises and from the performance of the governments against whom the terrorists act.

The individual case studies are of high quality and deserve more attention than they have received in this review. In short, *Terrorismo E Violenza Politica* represents an auspicious beginning to an important series of books.

LEONARD WEINBERG

University of Nevada, Reno

How to Compare Nations: Strategies in Comparative Politics. By Mattei Dogan and Dominique Pelassy. (Chatham, N.J.: Chatham House Publishers, 1984. Pp. 185. \$11.95, paper.)

Hubert M. Blalock, in *Basic Dilemmas in the Social Sciences* (Sage Publications, 1984), has recently cataloged a series of "defects in our intellectual culture" which undermine the scientific credentials and academic integrity of social science research. These defects—terminological confusion, excessively detailed description, technical and ideological intimidation, and the failure to eliminate inadequate theory—afflict comparative studies as much as any of the myriad sub-disciplines now encompassed by the term social science. It is a serendipity, therefore, that no sooner has the malady been diagnosed, than the cure is at hand.

In *How to Compare Nations*, Mattei Dogan and Dominique Pelassy have constructed a succinct and unconventional guide to the conduct of comparative analysis and the construction of social science theory. They begin with a general

discussion of the purposes of comparative analysis—stressing the need for cumulative theory-building, stating their own preference for a functionalist theoretic framework, and arguing that overgeneralization is a minor evil compared to overspecificity. They would have comparative analyses begin in some theoretic context. Some of the possible starting points are described in eight chapters, each devoted to a principal analytical category of comparative studies. Among these are the concepts of social class, political culture, political clientelism, consociational democracy, and developmental crises. What is refreshing is that Dogan and Pelassy do not purport to say anything new about any of these concepts. Rather, they provide a series of systematic and comprehensive literature reviews, exploring each concept as it has been refined through successive applications of the comparative method. They craft insightful syntheses that highlight fruitful lines of inquiry and expose the sterile and unproductive. This is a timely remedy for a field overburdened by disconnected generalization and excessive neologism.

The remainder of the work addresses alternative strategies for structuring international comparisons and the derivation of conclusions and refinements of theory. This, too, is richly documented with illustrative examples and constitutes an excellent discussion of inductive reasoning and the interrelationship of theory and research.

Although their work envisions substantial reforms of conventional approaches to comparative studies, Dogan and Pelassy refrain from pointed criticism. The result is an appropriately liberal definition of the intellectual core of comparative studies; something which should represent a significant contribution to the further development of the field.

The book is an adaptation of *Sociologie Politique Comparative* (Paris: Economica, 1982). Although in the Forward the authors disparage their own ability to handle the English language, their style of writing and the general organization of the text are most pleasing. *How to Compare Nations* should be required reading for all first-year graduate students; its use at the undergraduate level would be a sign of educational professionalism.

GARY M. KLASS

Illinois State University

Soviet Local Politics and Government. Edited by Everett M. Jacobs. (Winchester, Mass.: George Allen & Unwin, 1983. Pp. 225. \$28.50.)

The 12 essays in this book cover selected aspects of political phenomena at the local level in the Soviet Union. They are revised versions of papers read in 1980 at the 2nd World Congress on Soviet and East European Studies. The 11 authors are from three disciplines in six Western countries.

The book's strength lies in the diversity of its resource base: traditional institutional analyses are complemented by election studies, sophisticated survey data of third-wave emigrants, first-hand field observations by Western scholars in the field and their use of the increasingly sophisticated studies by Soviet scholars. The book shows the field of Soviet local politics coming of age methodologically and analytically.

The book's strength also lies in shedding new light on old questions such as the Party's role in the Soviet political system. The traditional thesis of Party versus governmental organs appears prominently in five of this book's essays, the ones by Jacobs, Robert Hill, David Cattell, Carol Lewis, and Bohdan Harasymiw. Although the authors do not agree on the precise lineup of forces, they all see tension between Party and state as a central question, and Jacobs himself raises the possibility of a system breakdown if the tension is badly managed.

But other authors see a different relationship between Party and state at the local level. Speaking of mass political participation, L.G. Churchwood starts a different tack by observing, in the most theoretical chapter in the book, that "evaluation . . . by Western political scientists has largely depended on their ideological orientation . . ." (p. 45). If the "Soviet version of the Soviet model" is given some credence, then there may not be a contradiction between the simultaneous expansion of influence among Party, Soviet, and social organizations.

The two essays by Theodore Friedgut—"The Soviet Citizens' Perception of Local Government," based upon survey research among third-wave emigrants, and "A Local Soviet at Work: The 1970 Budget and Budget Discussions of the Oktyabr Borough Soviet of Moscow," based upon first-hand observation—are in general accord with Churchwood's admonition that what you see is partly a function of what you look at and how you look at it. A fuller discussion of Friedgut's model of Soviet political participation can be found in his *Political Participation in the USSR* (Princeton University Press, 1979), a work regarded as seminal by other authors in the volume under review. Stephen

Sternheimer's skillful development of a "propositional inventory" for a communications model of Soviet local politics is also in this vein. Sternheimer tests Western-derived totalitarian, pluralist, and bureaucratic models and finds each wanting in its own way.

The book's most significant weakness is the absence of a chapter comparing and contrasting data sources, methodologies, models, and how given issues are analyzed in different ways. In a rapidly developing field, it could define the field and orient further research efforts. As good as many of the chapters are, an integrating chapter would have realized the book's potential to be more than the sum of its parts. Even without that chapter, however, Jacobs's intentions of providing an introduction to Soviet local politics for the general reader and student and also of providing material interesting to the specialist have been amply achieved.

ARON G. TANNENBAUM

Lander College

Women, the Family and Peasant Revolution in China. By Kay Ann Johnson. (Chicago: University of Chicago Press, 1983. Pp. ix + 282. \$23.00.)

This is a well-written and meticulously researched account of women, the family, and Chinese Communist party (CCP) policy in China. Kay Ann Johnson begins her study from the premise that "marriage, family and kinship practices have defined and shaped women's place in Chinese society more than any other single set of factors" (p. 3). She then proceeds to present a chronological survey of CCP policy toward women and the family in the Chinese countryside.

The book is divided into four parts. Part 1 is a discussion and analysis of the position of women in Chinese society in the prerevolutionary era. Part 2 presents CCP policy toward women and the family during the Chinese revolution from the founding of the CCP in 1921 to the establishment of the People's Republic of China (PRC) in 1949. Part 3 focuses on family reform in the PRC during the early years from 1950 to 1953 with a detailed look at the implementation of the 1950 Marriage Law. Part 4 considers three selected events—collectivization, the Cultural Revolution, and the Anti-Confucian campaign—in the period after 1953, with an analysis of their consequences for rural Chinese women.

Johnson argues that the history of CCP policy toward women and family reform has been characterized by contradiction, compromise, and neglect resulting in at best only limited progress

toward the realization of women's rights and gender equality. Although theoretically committed to a Marxist platform of gender equality, the CCP found it expedient from the very beginning to softpedal women's issues, subordinating its program of marriage reform to the preeminent need to gain the support of a traditionally minded male peasantry. After consolidating power, the CCP remained disinclined to push for a fundamental restructuring of family roles. The zenith of CCP reform efforts, the Marriage Law campaign of 1953, evoked controversy within the highest levels of the Party and was met with widespread cadre resistance to implementation at the grassroots level. Johnson's depiction of the dilemma faced by a Marxist-Leninist party seeking a socialist revolution in a peasant society might be seen as yet one more vindication of the Menshevik position. But Johnson is critical of the classic Marxist view of women and marriage, particularly in what she terms as its Engelsian variant, as "extremely inadequate for understanding and trying to alter the dynamics of women's subordination" (p. 231). Labeling the Marxist perspective deterministic and mechanistic in its one-sided identification of productive labor as the key to female emancipation, Johnson maintains that the Marxist position ignores the influence of culturally defined patriarchal attitudes and an oppressive family structure as independent variables inhibiting the transformation of women's roles. Although not denying the existence of economic and structural constraints, Johnson argues, persuasively in my opinion, that political and ideological forces can affect the dynamics of societal change and that the CCP, rather than accommodating with traditional elements, might have chosen alternative policies that placed a higher priority on the goal of gender equality.

Women, the Family and Peasant Revolution in China is of interest to the political scientist as well as to the China specialist and the student of women's studies. Johnson does a skillful job of connecting women's issues to the twists and turns of CCP policy, demonstrating her knowledge of the Chinese political scene in the process. Johnson is also to be commended for her efforts to integrate material from political science, sociology, and women's studies with a view to making cross-disciplinary and cross-cultural comparisons. One wishes, however, that Johnson had pursued the (admittedly obscure) question of CCP decision making in more depth. One learns a great deal about the content of policy; less about the dynamics of the decision-making process. What factors, for example, motivated the CCP to launch the Marriage Reform campaign of 1953, and why did the promoters of the policy emerge ascendant over their detractors? Part 4 of the

book, moreover, is not as well developed as the preceding sections. The chronological format is partially abandoned for a selective issue focus that provides less of an interpretative framework and assumes some prior knowledge of Chinese politics. In addition, Johnson's concluding comments are by now out of date due to the sweeping policy changes instituted in the Chinese countryside in recent years (although her prediction of the return of infanticide has been confirmed by subsequent events). Nonetheless, judged as a whole, it is unlikely that this book will soon be outdated. Johnson's research makes a valuable contribution to our understanding of the relationship between women, the family, and CCP policy in China.

JEANNE L. WILSON

Wheaton College

Interest Groups and Development: Business and Politics in Pakistan. By Stanley A. Kochanek. (New York: Oxford University Press, 1983. Pp. xiv + 393. \$19.95.)

Stanley Kochanek is one of the few South Asia specialists who have succeeded in doing research on a topic in both India and Pakistan. The present volume is a companion to, and logical extension of, his *Business and Politics in India* (University of California Press, 1974). The subject has been neglected by political scientists, who have left it to the economists with their quite different orientation.

For his paradigm, Kochanek applies the structural-functional framework of development theory pioneered in the 1950s by Gabriel Almond. This drives him to rather negative conclusions about the degree of modernity and efficacy of Pakistani business associations:

Although the external forms of organization in Pakistan are based on western models and appear to be similar . . . their internal structure and behaviour tend to be quite different . . . characterized by highly personalized styles of leadership, intense internal conflicts based on primordial ties, inadequate resources, a lack of professionalization . . . and low levels of mobilization, participation and institutionalization. . . . As a result [they] have lacked internal cohesion and suffered frequent organizational breakdown . . . which have enabled the government bureaucracy to penetrate, direct, and control their activities. (pp. 87-88)

Big business has tended to avoid the Chambers of Commerce and Industry and rely on individual contacts with bureaucrats and politicians in order to protect its interests.

All of this would be not very surprising to students of third-world development except for the few, tantalizing contrasts that Kochanek provides of Pakistani versus Indian business organizations. If India has been more successful in overcoming the barriers of caste, region, and language to fashion a modern mode of access to, and representation in, government, can the difference be ascribed to culture, or is it largely the contrast in the two political systems, with India's being largely democratic and Pakistan's authoritarian for long periods? Kochanek does say that although the epoch of the elected Bhutto regime (1971 to 1977) saw business of all sizes coalescing to better defend themselves from the Pakistan People's party's nationalization measures, they subsequently reverted to the personalistic mode of access under General Zia ul-Haq's rule. The fact that the Karachi edition of the book omits chapter 16, "The Zia Interregnum," raises the important issue for scholars and publishers of how far different editions should be tailored to the perceived preferences of regimes in power. Perhaps an alternative resolution of this dilemma would have been for Kochanek to reduce the number of broad judgmental statements about successive Pakistan regimes and focus the book more narrowly on business organizations themselves.

A more basic consideration has to do with the appropriateness of the structural-functional framework in analyzing and evaluating non-Western social and political institutions. The book does not reflect the deluge of both academic and political criticism of the single-path prescription for political, economic, and social development in the past two decades. If one does not wish to accept the Marxist, Maoist, or environmentalist attacks, at least the findings of Myron Weiner's *Politics of Scarcity* (University of Chicago Press, 1962) could be incorporated if an author proposed to deal with the wider context of interest groups and development in Pakistan. Functionalism itself was designed in part to make cross-cultural comparison more valid, but Kochanek does not ask how—and how well—the functions normally performed by business organizations in the West are done in Pakistan, and what the results for the whole system are.

These questions notwithstanding, the book is extremely well researched in a country where a lot of the basic data are not as easily obtained as in the United States or India. No scholar's or library's collection on Pakistan will be complete without it.

THEODORE P. WRIGHT, Jr.

State University of New York at Albany

Political Support in Canada: The Crisis Years.

Edited by Allan Kornberg and Harold D. Clarke. (Durham, N.C.: Duke University Press, 1983. Pp. xvi + 448. \$36.75.)

This work collects 14 scholarly essays that assess Canada in recent years. Appraised are such topics as political support and group dominance, national identification and political support, the political socialization of political support, political support and the mass media, media agenda-setting in the 1979 nationwide balloting, the failure of the national party system, popular attitudes and intergovernmental shifts in health program responsibility, political support and environmental policy, the resurgence of Canadian nationalism, nationalist ideology in Quebec (with particular reference to the Roman Catholic religious variable), and the Quebec "independence" referendum of 1980.

Of special interest is the book's longest essay, Alan C. Cairns's examination of the 1980 through 1981 developments climaxing with the patriation to Canada of a constitution with a bill of rights (the Charter of Rights and Freedoms) and comprehensive amending formulas:

This package is portrayed as a matter of honor, as the federal government's response to the vague commitment for renewed federalism given by Ottawa to the Quebec people in the referendum campaign. Not only did the referendum fail to further Parti Quebecois constitutional objectives, but by a profound irony Trudeau has turned the referendum on its head. From his own participation and that of other federalists he has extracted from the victory of the 'no' forces a mandate to implement the precise reforms of Canadian federalism he has been pursuing for more than a decade. (p. 438)

Former Prime Minister Trudeau, Cairns recounts, long opposed the fostering by provincial governments of provincial cultures and economies for which they could speak exclusively. Rights entrenched in the Charter and enforced by a federal Supreme Court "inevitably will be employed as a resource by a future prime minister in the never-ending power struggle between federal and provincial governments" (p. 432).

Cairns records that the extent to which the Trudeau government deliberately sought this consequence was unclear, but adds that "Trudeau has long and patiently awaited the opportunity to implant his vision of Canada on the fragmented country he inherited" (p. 443).

The volume overall presents a worthwhile body of work.

GEORGE STEVEN SWAN

St. Thomas University

Islam in the Political Process. Edited by James P. Piscatori. (New York: Cambridge University Press, 1983. Pp. 240. \$37.50, cloth; \$14.95, paper.)

The essays in this volume stem from a 1981 conference at the Royal Institute of International Affairs at Chatham House, England. The book is the result of a research project funded by the Ford Foundation on the impact of Islam on the international system. The 10 essays, the Introduction, and the Conclusion examine the role of Islam in the domestic politics of 11 selected Muslim countries: Algeria, Egypt, Indonesia, Iran, Iraq, Pakistan, Saudi Arabia, Senegal, Sudan, Syria, and Turkey. In most cases, well-known scholars look into the circumstances and historical background that have made Islam significant in those countries.

Islam, the religion of the overwhelming majority of the people of the countries under study, has a guiding and often decisive influence on the conduct of governments and individuals. It is significant that the non-Islamic world has belatedly discovered the Islamic revival only after the Iranian militants sought the late Shah's return to face Islamic justice. The fact that the Shah's supplanter seems to have succeeded by appealing to religious sentiment has astonished Western onlookers, who take it as axiomatic that religion and politics have little to do with one another.

In addition to editing the volume, Piscatori contributes the Introduction and chapter 4. In the Introduction, he delineates the various approaches to the study of Islamic politics, conservative and scientific. A 10-page introduction is at a definite disadvantage when it purports to examine various aspects of a world religion, yet Piscatori has managed to write a useful outline of the 10 essays on the internal political dynamics of the various nations.

The essay on Egypt argues that religion is emasculated by the state. The chapter on the Sudan traces the evolution of a "consensus" calling for the establishment of an Islamic state. The essay on Saudi Arabia focuses on the blending of tradition and modernity; that on Syrian and Iraqi politics maps out the various norms and values of Islam in the two countries. The essay on Algeria examines how Islam is used by both the government and its opposition. The chapter on Senegal narrates Sufiism as a mode of Islamic devotion. The seventh chapter shows that religion is shaped simultaneously by social, political, and economic factors in Turkey. The essay on Iran highlights Khomeini's concept of "guardianship of the juriconsult." The next essay deals with the Islamization process as a bond of unity in the historically unstable country of Pakistan. The tenth chapter

describes the various problems that Islam faces in consolidating Indonesia.

The concluding essay summarizes the common themes of Islamic politics: the urbanization of Islam, the use of the religion in the political jargon of both governments and their opponents, and the role of religious institutions such as the mosque and the religious school as intermediary between the individual and the state.

There is no single approach in these essays, and the authors have called upon the insights of various disciplines. However, all of the authors are convinced that the idea of Islamic politics needs to be specific if it is to be made more meaningful and creditable in the contemporary world.

Throughout this detailed and thoroughly documented study, the reader finds an erudite and intricate exegesis of Islamic principles and doctrines. Although an integrating essay would have improved the volume, it is a serious study meant for scholars and advanced students of Islamic politics and government.

SHEIKH R. ALI

North Carolina Central University

Juan Peron and the Reshaping of Argentina.

Edited by Frederick C. Turner and Jose Enrique Miguens. (Pittsburgh: University of Pittsburgh Press, 1983. Pp. xii + 268. \$24.95.)

This collection is a serious attempt to assess dispassionately (if in a friendly fashion) some of the key aspects of the nature and consequences of the Peronist governments and movement. Contrary to its title, the book is primarily about Peronism and not Peron. Most of the authors, both Argentines and North Americans, are established scholars with considerable background on Argentina. They present both reviews of the existing literature and original research and insight.

The articles in this collection have more cohesion than is generally the case in edited works. However, a weakness of the book is the failure of the editors to make that cohesion more explicit. At least three themes stand out. The first is rationality—in contrast to so many works on the movement. Peronism is seen as growing out of and reflecting the forces shaping and dividing Argentina. The persistent support of the urban working class is seen as a rational response to Peron's incorporation of them into the political process and his attempt to raise their real income. Evita is presented as hard working and successful, at least in the short run, in aiding those most in need of help. Peron himself is portrayed as an able politician rooted in Argentine traditions—albeit, also, a very human man with many flaws.

A second concern is the composition of the Peronist movement. Jose Miguens uses survey data from the 1973 elections to demonstrate the strong support of the working class for Peronism in preference to the parties of the Left. His data also show that although Peronism draws its greatest support among the working class, it is in fact a multiclass movement. In an exceptionally well-done essay, Manuel Mora y Araujo and Peter H. Smith use aggregate data to demonstrate that after 1946 a significant negative correlation developed between level of development and Peronist vote. In industrial areas, the Peronist vote is strongly class-based, whereas in less-developed regions, poor social conditions and traditional political elite influences contributed to Peronist success.

Third, Peronism is seen as both a source and a reflection of the conflict that has racked Argentina for the last 40 years. The most noticeable source of instability is the inability of successive governments to come to grips with Peron's inclusion of the working class as an active participant in the political and economic systems. In a very perceptive essay, Gary Wynia compares the incomes policy strategies used by military, democratic non-Peronist, and Peronist governments after 1955 and concludes that each of three strategies—autocratic, bargaining, and cooptation—achieved modest short-term success but eventually failed, particularly for political reasons. Antonio Donini shows Peronism reflecting Argentina's religious divisions.

The weaknesses of the book are more in its editing than its substance. The introductory chapter and the Epilogue do too little to pull the collection together. Although the thrust of his argument seems sound, Miguens's critiques of the misinterpretations of Peronism tend to be too brief to be convincing or are just as strained as the argument criticized. Roberto Guimaraes's analysis of public support for terrorism as reflected in survey research raises serious questions about several common explanations of terrorism such as relative deprivation. However, the article has several weaknesses that could have been eliminated easily. For example, on page 197, Guimaraes improperly adds the variance explained by two separate multiple regression equations but then on the following page essentially corrects himself.

The book as a whole will be of value to professionals and graduate students interested in Argentina, Peronism, and populism, and several chapters would make excellent assigned reading for undergraduate or graduate courses—especially the chapters on Evita, incomes policy, religion, and voting patterns. Contemporary historians should find a useful source in Wayne

Smith's highly detailed account of the events in 1971 through 1973 leading to the return of Peron. The book contains endnotes, an index, a chronology of Peron's public career, photographs, and tables.

ROBERT E. BILES

Universidad de los Andes (Bogota)

Brazil in Transition. By Robert Wesson and David V. Fleischer. (New York: Praeger Publishers, 1983. Pp. iv + 200. \$24.95, cloth; \$10.95, paper.)

Brazil in Transition is the latest volume in the Hoover Institution's series on politics in Latin America. Like the other volumes in the series, it seeks to provide an overview of Brazilian politics for those with little background in Latin American studies.

Brazilian politics have undergone monumental changes in the past decade, and this volume provides a useful analysis of contemporary political events. As such, the volume is very useful for those individuals who have not kept abreast of the rapidly evolving Brazilian political scene.

The book's weakest point lies in its lack of an explicitly theoretical approach. The brief historical introduction provides little more than a glimpse of the complex forces shaping Brazilian political history. For example, the authors discuss Brazil's national political styles in terms of *coronelismo*, clientelism, and mass political styles, but fail to carry these useful conceptualizations through the rest of the book.

What Wesson and Fleischer lack in theoretical perspective, however, they make up for in their impressive display of knowledge of the contemporary Brazilian political scene. As the title of the volume indicates, they are primarily concerned with exploring the prospects and forces for political change. The vagaries of the *abertura* process are presented in fascinating detail, with particular attention to the complex electoral maneuvering of the military government in attempting to control the democratization process.

Wesson and Fleischer successfully blend historical and current material in their treatment of both political parties and the bureaucracy. Contending political and institutional goals of the bureaucratic agencies, for example, are discussed in greater depth than most introductory analyses of Brazilian politics. Perhaps reflecting the authors' years of experience in Brazil, the reader is left with the impression that they are well versed with the nuances as well as the formal political structures of Brazilian politics.

The volume is also useful in its treatment of dif-

ferences among military regimes. Paralleling current trends in scholarship on Brazil, the armed forces are not portrayed as a monolithic entity; rather, Wesson and Fleischer focus on the more interesting questions of divergence between military factions and regimes.

Brazilian-U.S. relations are analyzed in the historical context of U.S. support for and participation in the 1964 military coup. The ebb and flow of the U.S.-Brazil axis are traced carefully, with attention paid both to areas of convergent interests between the two nations and to the numerous cases of disagreement between the two governments. Wesson and Fleischer explain Brazilian foreign policy as "pragmatic, frankly self-interested, and resolutely nonideological, identifying neither with the West nor the Third World" (p. 164). Unfortunately, only minimal attention is devoted to Brazilian foreign policy toward the Third World—an area of growing political and economic significance to the government.

In the final chapter, Wesson and Fleischer underscore the volume's theme: *Abertura* is the inevitable result of the pressures generated by both the economic growth of the early 1970s and the recession of the early 1980s. The interrelationship of political and economic pressures is carefully and sensitively drawn.

Overall, this is a well-written book that will be most useful to both beginning students of Brazilian politics and to scholars who have not revised their views of Brazil since the *abertura* process acquired its own dynamic several years ago. The volume's sketchy historical treatment and theoretical superficiality preclude its emergence as a pioneer in the field of Brazilian politics. Although this volume will never have the impact of, say, Peter Evans's *Dependent Development* (Princeton University Press, 1975), it serves an important function for those wishing a sophisticated overview of the Brazilian political system.

ELIZABETH G. FERRIS

Lafayette College

The Labour Party in Crisis. By Paul Whiteley. (New York: Menthuen, 1983. Pp. ix + 253. \$25.00, cloth; \$12.95, paper.)

Paul Whiteley begins his study on an ominous note: "The Labour Party is in serious trouble, and faces a series of crises greater than it has experienced since the 1930s" (p. 1). Although his book was at proof stage when the 1983 general

election was called, the outcome of this most recent British election simply serves to confirm Whiteley's pessimistic diagnosis. As he points out in a postscript analyzing the 1983 outcome, Labour's share of the popular vote fell to its lowest level since 1918, and the party won just 20.6% of the registered electorate.

The first part of this book is devoted to a close analysis of the series of crises to which Whiteley refers at the book's beginning. The first of these is the persistent but recently sharpened ideological crisis between left and right within the party that precipitated the formation of the breakaway Social Democratic party in 1981. Within the context of this discussion, Whiteley offers a particularly useful analysis of ideological patterning and constraint among Labour supporters and the Labour elite, charting a growing shift to the Left within the party. The second of the crises concerns individual party membership, which Whiteley suggests has been falling at an average annual rate of 11,000 over the postwar years. The third crisis results from the more immediately evident electoral decline of the party, particularly since the 1960s.

All three crises are clearly interrelated, and Whiteley's central thesis is that it is "failures of policy performance" (p. 1) that lie at their heart. The second half of the book accordingly sets out to analyze those failures, focusing in particular on the economic and welfare record of Labour in government.

Whiteley's overall argument is a plausible one. Throughout his analysis of the crises facing Labour he emphasizes the instrumentality or contingency of the party's support, suggesting a model of British voting in which the central assumption is that "voters support a political party because it brings them objective and subjective benefits at the individual and collective levels" (p. 90), although later speaking of "an instrumental electorate motivated by performance rather than promises" (p. 104). By then drawing attention to Labour's failures in office, Whiteley manages to construct a convincing explanation of the party's contemporary malaise.

It is not Whiteley's intention to suggest that poor policy performance is the one and only source of Labour's decline. Rather, by moving away from traditional sociological explanations, he acts to place the responsibility for decline more squarely on the shoulders of the party itself instead of seeing it as the more or less inevitable outcome of socio-structural transformation. What Labour does then becomes at least as important as what happens in the society around it. In this sense, Whiteley's study is a valuable attempt to redress the balance of explanations of British political change and as such, should be read by

the practitioners as well as the students of Labour politics.

PETER MAIR

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National Communism. By Peter Zwick. (Boulder, Colo.: Westview Press, 1983. Pp. x + 249. \$28.50, cloth; \$12.00, paper.)

Peter Zwick treats in this book the relationship between nationalism and communist movements. The first third of the volume is devoted to this relationship from Marxism's inception to the dissolution of the Comintern in 1943; the remainder deals with the rise of "national communist" regimes and parties since World War II.

Zwick displays a lively prose style and command of a remarkably broad variety of source materials. Certain sections, such as those on Marx's wrestling with the national question and the Yugoslav road to communism, are particularly insightful. But these merits are probably outweighed by the book's obvious flaws, some of which are attributable to its overly ambitious scope.

Zwick's thesis is that "national communism" is the only viable form of communism and that this has been true from the origin of Marxism. However, the connection between nineteenth-century Marxism and the behavior of contemporary communist party-states is not entirely clear. Moreover, the analysis in the early chapters poses a problem for the entire volume. Despite an exhaustive examination of communism and nationalism, these key concepts remain ambiguous.

More troubling than ambiguity of the two key concepts is treatment of the next level of concepts. Zwick seems mostly to accept at face value the rhetoric of communist spokesmen, and such terms as imperialism, exploitation, and national liberation are used uncritically. True, there is a late clarification on "imperialism" (p. 218) and an acknowledgment that communist systems can also be imperialistic (p. 227), but by then the damage has been done. Generally, the approach to communist systems comes across as rather naive. Pre- and noncommunist regimes are described in emphatic terms as oppressive, but there is little commentary about communist oppression, particularly in regard to national liberation movements.

Notably, Zwick attributes the dominant role of Kim Il-sung in North Korea, where communist totalitarianism is probably more pronounced than anywhere else in the world, solely to that leader's popularity (p. 181). Further, it is rather curious that Zwick's main authority in his interpretation

of Castro is Herbert Matthews. Surely the latter's murky role as a legitimizer of Castro in the 1950s should raise some questions about his reliability as an appraiser of the Cuban leader in the 1960s.

In general, Zwick places too much emphasis upon the factor of popular support in communist systems and devotes insufficient attention to the central role of coercion in *all* party-state regimes. There are also two particularly puzzling problems in his wide-ranging survey. First, although Soviet nationality policy under Lenin and Stalin is covered in detail, there is no consideration of nationality conflicts within China. Second, on Zwick's showing, Nicaragua should not be included under the rubric "national communism," but it is nevertheless so included, and it is not clear why this is done.

The dichotomy between nationalism and international communism surely does not fully explain communist behavior. Zwick omits factors in communist party-state politics other than nationalism,

and the argument on the singularity of national communism does not account for obvious linkages and conditions of dependence. Finally, a consideration of contemporary Soviet integration theory, strongly reasserted in the 1970s and apparently without theoretical limits, would have been most instructive.

One can only suspect that a stronger lead from readers and editors on matters of scope and direction would have resulted in a vastly different and better book. Zwick possesses a truly encyclopedic knowledge of the history of Marxism and, given free rein to develop his thesis without spatial limitations, or alternatively, compelled to adopt a more narrow behavioral focus, he might have fulfilled the expectations aroused by the dazzling brilliance of the book's opening passages.

R. JUDSON MITCHELL

University of New Orleans

International Relations

Arab Petropolitics. By Abdulaziz Al-Sowayegh. (New York: St. Martin's Press, 1984. Pp. 207. \$25.00.)

The thesis of this book, that "the oil crisis is rooted in the Palestinian question rather than in the seeming imbalances between the supply of and demand for oil" (p. 192), is outlined under four subjects: (1) the relationship between the Arab-Israeli conflict and Arab use of oil as a political instrument; (2) the development of Arab oil strategy; (3) the determining factors in the use of oil power; and (4) the opportunities for, and limitation of, the use of oil power.

In *Arab Petropolitics*, Al-Sowayegh attempts to examine the relationship between oil and politics through a case study of the Arab-Palestinian-Israeli conflict. He contends that this political problem and the oil crisis are synonymous. This is an erroneous assumption because the Palestinian identity did not occur recently, nor is it yet complete. It has been vague ever since the Romans coined the term in the second century.

Equally puzzling is Al-Sowayegh's conclusion that the oil crisis is linked inextricably with the Arab-Israeli conflict, a conclusion that ignores the roots of the two incompatible nationalisms—Palestinianism and Zionism—which produced the troublesome problem for Palestine. It is important to remember that the direct Arab interests in Palestine began to develop only with its conquests

more than 13 centuries ago when the Palestinians became Arabized.

It is well known that the petroleum industry began to expand in the nineteenth century when none of the Middle Eastern countries even knew that oil as a commodity existed in that part of the world. Al-Sowayegh is right when he says that contemporary oil politics reveals a pattern in the control of oil in the Middle East as functional to the power politics of the day. In this regard, he focuses on the major forces and issues concerning oil power and deals with the three-way jousting among the oil-producing countries, oil companies, and consumers.

Al-Sowayegh indicates that today's energy crisis in the world was precipitated, but not caused, by the Arab-Israeli War of 1973 and the resultant embargo. The geometric growth in demand for energy in the world had drawn far too little attention as the finite supplies dwindled. When the Arabs reduced production and imposed a world-wide oil embargo they also, as Al-Sowayegh points out, stepped up their efforts through diplomatic channels to have Israel withdraw from the Arab-occupied lands. Since then, the Middle East has continued to be the battleground among the powers of the world.

In an attempt to vindicate the Western charges that the Arab oil "embargo contradicted every existing economic rule" (p. 136), Al-Sowayegh abandoned his own position by saying that

"although the cutback in oil exports was exclusively a political decision, oil price rises were a mixture of both political and economic factors" (p. 136).

A fear of Israel and a desire to prevent radical incursion into the Gulf region prompted the Arab countries to emerge from their insularity by joining the militant Arabs in the use of oil as a weapon in the Arab-Israeli-Palestinian conflict. This is patent when Al-Sowayegh concludes his study by saying that "the main threat to the Arab nation is Israel, and for this reason the Palestinian question will remain the most important security issue for the Arabs and the world" (p. 193).

Despite imperfections, Al-Sowayegh has written an easily understood account of Arab oil politics that should be useful for those wanting to know more about the Arab view of Middle East turbulence and the world oil crisis.

SHEIKH R. ALI

North Carolina Central University

African Security Issues: Sovereignty, Stability, and Solidarity. Edited by Bruce E. Arlinghaus. (Boulder, Colo.: Westview Press, 1984. Pp. x + 229. \$25.00.)

The majority of the chapters in this volume stem from a panel on Inter-African Security Issues at the 1982 Annual Meeting of the African Studies Association. Three major security issues are addressed: (1) sovereignty, that is, the "ability to achieve economic and political self-sufficiency free from foreign or domestic interference"; (2) stability, "the creation and maintenance of a viable political environment in which orderly and progressive social and economic change may take place"; and (3) solidarity, "the development of intraregional interdependence and cooperation to foster both sovereignty and stability as well as a more significant and beneficial role for Africa in world affairs" (p. ix). These clearly are three crucial issues, although the point about "foreign or domestic interference" in the context of national sovereignty seems somewhat muddled, and the topics of interregional interdependence and sovereignty are somewhat at odds.

The sovereignty issue is examined by Arthur J. Klinghoffer in "The Soviet Union and Superpower Rivalry in Africa," and by I. William Zartman in "Africa and the West: The French Connection"; Wayne A. Selcher and Pauline H. Baker present studies of Brazil-Africa relations and of Nigeria. A contradiction emerges. On the one hand, the suggestion is strongly advanced by some of these authors that superpower rivalry

somehow is imposed upon unwilling countries anxious to preserve their sovereignty. On the other hand, Arlinghaus reminds us (p. 2) that if African leaders opt "to mortgage their future for a modicum of security today, it . . . is *their* decision to make." Precisely. Moreover, the immense and spreading corruption and the mismanagement accompanying arms transfers, neither classifiable as security measures, are not sufficiently reflected in this volume. One must also question whether fulfillment of genuine security needs from superpower arsenals is as dire a threat to African sovereignty as several of the authors seems to assert. Were not Soviet military and civilian personnel unceremoniously expelled from at least three African states whose armed forces had been built up with Soviet assistance?

Ambassador Deng, noting the disutility of the OAU Charter when it comes to African security, contends that the drain on scarce resources caused by security needs inevitably leads to diminution of national sovereignty. Does that necessarily follow? But Deng is correct: The OAU Charter definitely does not protect the sovereign independence and national security of a steadily increasing number of African states. Klinghoffer, in a very informative chapter on Soviet interests and motivations, finds that Moscow's tactics in Africa are "reactive rather than primary" (p. 21), a point shared by numerous observers throughout the world, but not necessarily accepted by Washington.

In a richly documented, somewhat theoretical contribution, "Social Incoherence and the Mediatory Role of the State," Donald Rothchild draws attention to the wide range of options beleaguered governments actually have when security requirements must be met from scarce resources. Treating the military as a political force capable on occasion of genuine ideological concerns, Claude Welch examines the bearing of civilian-military relations on political stability. Victor Olorunsola and Dan Muhwezi address "Security and Stability Implications of Ethnicity and Religious Factors." Their finding of the now-familiar fact that "ethnic heterogeneity in some countries is not conducive to stability" (p. 154) does not seem worth the effort. Likewise, it is also well known that "intense competition for power and public goods and the often resulting maldistribution of those scarce resources have created conditions that may easily lead to conflict" (p. 154). Is there a country on this planet where that is not the case?

On the subject of African solidarity, Annette M. Seegers sees "South African Liberation" as the "Touchstone of African Solidarity." Is it really? To her credit, Seeger recognizes that agreement among African states at the level of rhetoric

does not necessarily spell agreement on precisely how or when that liberation is to be achieved. Finally, D. Katete Orwa, in "National Security: An African Perspective," concludes that a prime threat to the national security of a growing number of African states is the growth of their armed forces. Pessimistically, he foresees that the pressure emanating from that direction may overwhelm what conflict resolution capability the OAU may have left in its progressively weakened state. From this, as he sees it, arises an urgent need for return to professionalism in domestic and foreign policymaking, in particular a turning away from the "non-professionals" (p. 210), presumably politicians "in and out of uniform." A faint voice in an expanding wilderness.

On balance, given the global strategic implications of security and stability on the African continent, this volume is a welcome addition to the growing body of national and international security studies.

HENRY L. BRETTON

State University of New York College at Brockport

Arms for Africa: Military Assistance and Foreign Policy in the Developing World. Edited by Bruce E. Arlinghaus. (Lexington, Mass.: D.C. Heath, 1983. Pp. xiv + 233. \$26.95.)

For the past 10 years, since the publication of Robert Harkavy's *The Arms Trade and the International System* (Ballinger, 1975), there has been a small but steady stream of books examining the arms trade, particularly as it involves major power suppliers and third-world recipients. Relatively few authors, however, have explicitly addressed the extent to which governmental arms transfers serve as an instrument of foreign diplomacy, particularly through providing the supplier with direct or indirect influence over recipient state actions and attitudes. Equally rare is a focus on transfers to sub-Saharan African states.

The importance of this edited volume, which began in 1981 as a panel at the African Studies Association meetings, is that the authors of the individual chapters have taken on the challenge of these two tasks. Included are six case studies of arms transfers policy—Soviet (Edward J. Laurance), Eastern European (Roger E. Kanet), Chinese (George T. Yu), French (Edward A. Kolodziej and Bokanga Lokulutu), German (Regina Cowen), and United States (Joseph P. Smaldone)—as well as more general discussions by David E. Albright and Cynthia Cannizzo and an analytic framework set out by Arlinghaus in the introductory chapter. Not all the chapters are equally enlightening; however, on balance this is

an interesting and worthwhile contribution to the arms transfers and foreign policy literatures.

For the most part, the chapters are solid descriptions of the motivations of suppliers in providing military assistance to African countries. Several contain brief discussions of specific supplier-recipient relationships (e.g., China/Tanzania; USSR/Zimbabwe; France/South Africa); many report the extent and types of military assistance provided by supplier states. The balance between "push factors" and "pull factors" is also addressed.

In addition to examining supplier incentives, several authors speculate on the probable future of arms transfers to Africa. Conclusions are mixed; however, the general consensus is that although African states will continue to import arms, the rapid growth in acquisition which was evident in the 1970s has probably ended, restrained by a combination of domestic variables such as high interest rates, the high level of African debt, the problem of underdevelopment, a slight lessening in racial conflict in Africa (which was part of the reason for early arms transfers), and the possibility of a Pan-African Defense Force. In addition, there is likely to be a reshuffling of suppliers, with secondary European states and third-world countries gaining a greater portion of the market than was true five or 10 years ago.

Although there is much that is valuable in this collection, the book is not flawless. One weakness is that several of the chapters do not directly focus on the supposed theme—the question of influence—but instead limit themselves to the far simpler task of general discussions of supplier patterns and motivations. A greater degree of "connectedness" among the separate pieces would have been helpful. A more serious disappointment is the very limited use of quantitative research on the issue of arms and influence. None of the chapters breaks out of the descriptive/analytic mold, and only rarely do the authors even report relevant empirical studies conducted by others (Smaldone being one exception). To be sure, problems with obtaining reliable arms transfer data makes such research more difficult; nonetheless, at least some attention to the insights of this mode of analysis would have strengthened the book considerably. Despite these reservations, this book is well worth the attention of scholars of arms transfers and foreign policy, for whom it should stimulate further debate and research on a much needed general theory of arms transfer policy.

DEBORAH J. GERNER

Hamilton College

Linkage Politics in the Middle East: Syria between Domestic and External Conflict, 1961-70. By Yaacov Bar-Siman-Tov. (Boulder, Colo.: Westview Press, 1983. Pp. ix + 176. \$18.50, paper.)

Bar-Siman-Tov examines connections between Syria's external and internal conflict through his development of Rosenau's concept of "linkage politics." In this study, Syria's "linkage groups" have ties to one or more of her neighbors and affect the relationship between their state's external and internal conflict (e.g., increased domestic conflict leads to increased foreign conflict).

The period of Syrian politics under study runs from the September, 1961 breakup of the Egyptian-Syrian Union to the Asad assumption of power in November, 1970 and is subdivided into three segments: the Separatist regime from 1961 to 1963; the Ba'th regime from 1963 to 1966; and the neo-Ba'th regime from 1966 to 1970. During this period, Syria was a polity with chronic severe political instability whose governments either created or sought to use existing or imagined foreign conflict in order to reduce their country's domestic conflict, although usually with little success. This study gives no support to leaders who hope that conflict abroad will bring long-term unity to a badly divided country.

Bar-Siman-Tov argues persuasively for concentrating on a single state as well as for relying on a blend of quantitative and qualitative (historical) analysis when studying external-internal conflict relationships and their connection with linkage groups. Bar-Siman-Tov's use of Syrian documents, prior research, and press and radio accounts provide an apparently adequate factual base for his descriptions of event patterns and delineations of group activities. Although specialists on Syrian politics may question some of his points, impressive evidence is presented for his four variants of the external-internal conflict relationship as well as for the critical role played by linkage groups in them. After reading this book, the results of the multination quantitative studies that show little or no relationship between external and internal conflict appear even more dubious than ever. Scholars interested in external-internal conflict and/or linkage politics should pay careful attention to this study.

Nevertheless, this short volume is mildly disappointing. Despite a solid although not very original critique of the lack of theory in the multination, quantitative studies, Bar-Siman-Tov has created what is closer to a descriptive research outline than a well-developed theoretical framework. His "proposed method of research" chapter not only lacks explicit hypotheses for future testing, but also appears to have been

created at least as much to suit his Syrian example as for its future utility. Furthermore, without evidence, he brushes aside as hopelessly impractical even the possibility that the conditions that determine the links between external and internal conflict can be stated beyond the point he has reached. Although this book correctly identifies and intelligently discusses a number of the elements of a possible theoretical framework, it is not as developed in theoretical terms as it could have been.

Also, it is regrettable that apart from a discussion of Syrian politics and, to a lesser extent, of Egyptian-Syrian relations, the material is not used to shed much additional light on Middle Eastern politics. Thus, this volume contains relatively little to attract persons with a general interest in the area.

Finally, the last chapter is perfunctory and presents only a brief, sketchy summary of points dealt with earlier. It appears to have been added for the sole purpose of giving the book a formal conclusion.

Despite these limitations, this is a valuable, original study. It should be noted that it contains an index and complete footnotes, but no separate bibliography.

JOSEPH M. SCOLNICK, JR.

Clinch Valley College, University of Virginia

The Politics of Nuclear Balance: Ambiguity and Continuity in Strategic Policies. By William H. Baugh. (New York: Longman, 1984. Pp. xii + 276. \$27.50, cloth; \$17.95, paper.)

William Baugh has written a valuable road map for the reader interested in understanding the welter of indexes, statistics, and models that mark the intellectual landscape of the nuclear arms competition. These measures comprise what is typically termed the strategic balance. The analytic elegance of these techniques, however, frequently masks a far more vital question. Can we accurately ascertain an imbalance in the relative performance and strength of the Soviet and American arsenals? The answer is no. The reason, according to Baugh, is the lack of agreement "either within or between governments on how it [the balance] is to be measured" (p. 2).

Baugh examines the range of methods with clarity and insight. Beginning with simple measures such as numbers of weapons and total megatonnage, he proceeds to explain such performance parameters as silo hardness and warhead accuracy. He then turns his attention to various figures of merit, for example, Total Equivalent Megatonnage (EMT), a gauge of area destruction,

and Hard Target Kill Potential, a calculation of the vulnerability of missile silos to attack. Finally, Baugh critiques various nuclear effect models used to calculate the number of residual weapons surviving a notional nuclear exchange. Such models offer a dynamic way of testing weapon performance and alternative targeting strategies. Of the three models discussed, NEMATODE 2.2 was designed by the author and serves as the basis for his subsequent examination of the growing vulnerability of land-based ICBMs to a preemptive attack. Baugh concludes, on the basis of his calculations, that the "window of vulnerability" that opened for the United States in 1980 and 1981 will open for the Soviet by mid-decade even without the MX. Significantly, this judgment only makes sense within the context of a counterforce scenario. In Baugh's view, there is no "open window" for either superpower in maintaining some minimum level of deterrence.

The value of this book is less certain for those specialists in the field who are interested in the mutual interaction between Soviet and American nuclear strategy. The reason can be traced as much to Baugh's conceptual approach as to his almost complete reliance on secondary sources for his analysis. His approach is tethered to an elementary exercise in game theory—the prisoner's dilemma. In this highly simplified game of strategy, two opponents are more likely to pursue a new weapons program rather than practice restraint because neither trusts the other and each wants to cover every contingency. The logical outcome of this interplay between fear and technology would seem to be a growing convergence in the strategic doctrines as well as the force postures of the superpowers. In Baugh's words, "having reached what is generally regarded as rough nuclear parity, the two superpowers have gravitated to the same region of doctrinal space" (p. 74). In support of this proposition, he cites a 1971 essay by Roman Kolkowicz. In fact, Baugh relies almost exclusively on this essay whenever he attempts to explain the Soviet perspective. The proposition that fear and technology has led to doctrinal convergence may be valid, but the assertion demands far more evidence than is presented in this book. The absence of a well-researched discussion of the Soviet perspective on military doctrine and strategy leads Baugh to erroneously ascribe an "evolved [triad] doctrine" (p. 87) to the Soviets and to ignore the differences in the Soviet and American approaches to the problem of civil defense.

Baugh concludes his book with a speculative look at the future prospects for arms control. He envisages a series of small cooperative steps as the most profitable approach to the problem of reducing the nuclear threat. Regardless of the

magnitude of the step, progress will depend upon some semblance of agreement between Moscow and Washington on the nature of the strategic balance. This will require an even greater effort to understand the Soviet calculus for the nuclear equation.

PAUL MICHAEL KOZAR

Washington, D.C.

Thinking about National Security: Defense and Foreign Policy in a Dangerous World. By Harold Brown. (Boulder, Colo.: Westview Press, 1983. Pp. ix + 288. \$16.95.)

This is a thoughtful and persuasive analysis of American national security policy. Although occasionally technical, the book is designed for the general audience interested in foreign and defense policies.

Brown attempts to cover 16 issues in 280 pages, so some of the analysis is rather sketchy. But the strengths of the book clearly outweigh its weaknesses.

Throughout the book, Brown effectively expands upon his definition of national security: "Security must depend on the nation's internal political and economic strength; the will of the people and their ability to persevere in a given course; the quality of U.S. education and technology; the state of national leadership; and the degree of confidence the public has in that leadership" (p. 262).

One crucial issue that has impact upon the security of the industrialized democracies is the crisis of political authority. Brown attributes the cause to the attitude that peace and steady improvement in living standards are inevitable. "This attitude makes the industrialized democracies fragile in the face of adversity, and a lack of resolve could well erode the willingness of their people to make sacrifices to preserve the democratic system and the market economy" (p. 25). This argument is persuasive as far as it goes, but surely the issue is more complex than that. Other causes relate to the dispersion of political influence, increasing inefficiency of government, citizen alienation, and the internationalization and externalization of policy responsibilities.

Brown's discussion of the all-volunteer force and the possibility of conscription is thought-provoking. He argues that: "It is important to begin working now on plans for possible substitutes for the all-volunteer forces, especially those that would involve obligatory service, to ensure that they meet concerns about equity, universality, constitutionality, and need. This will enable us to avoid unfairness and mass divisive-

ness should such a program prove necessary" (p. 259). To those who propose universal service for young men and women, Brown suggests a "defense tax," which would impose some sacrifice upon the entire population and would help to justify to young people the service required of them.

Brown supports one line of reasoning that is clearly popular, but it seems quite problematical. That is: "A military balance viewed by the U.S. Government and public or by U.S. allies and friends as unsatisfactory—or even as becoming less favorable—raises the possibility of explicit or implicit Soviet political intimidation" (p. 265). Perhaps this is characteristic of many "truisms" in political science. But such generalizations are difficult to prove in concrete circumstances, and their use is primarily polemical.

ROBERT H. PUCKETT

Indiana State University

South Africa in Southern Africa: The Intensifying Vortex of Violence. Edited by Thomas M. Callaghy. (New York: Praeger Publishers, 1983. Pp. viii + 420. \$45.00.)

For roughly the past quarter century, South Africa has attracted international attention, as well as opprobrium, primarily because its ruling elite attempted to divert what British Prime Minister Macmillan, in his address to both Houses of Parliament in Cape Town, referred to as the "winds of change." This oligarchy has proven resilient, innovative, resourceful, and willing to use coercion to sustain its style of privilege and affluence which it masks either in terms of Christian guardianship, or in Kiplingesque phrases concerning the trusteeship responsibilities of the white body politic. Its detractors regard the regime as the apotheosis of racism, or even the best friend the Soviets and their allies ever had in terms of a seedbed for disaffection and possible cataclysmic revolution. Those who find the Manichaean approach unappealing look at the regime in Pretoria as a modernizing oligarchy, or as a regime which faces, and can contain, low-intensity conflict. Others operate in a larger continental or global frame of reference.

In the fall of 1980, Pennsylvania State University sponsored a conference on Southern Africa just before the annual meeting of the African Studies Association in Philadelphia. The timing enabled the University to attract a wider group of participants than might otherwise be possible. Thomas Callaghy undertook the arduous task of editing the conference papers for publication. It is not clear from the Preface (p. v) whether all of the

papers presented at the 1980 conference are included in the edited volume. Callaghy divides the book into two parts, the first titled "Regional Orientation and Impact," the second, "Black States and Groups in Southern Africa." The authors are well-known, established Africanists, most of whom are teaching in American universities.

Not surprisingly, the bulk of the 14 essays concentrate on one nation-state, or on bilateral relations between South Africa and one or more of the states, homelands, or territories in the region. Some of the essays treat the region as a whole, for example, the Southern African Development Coordination Conference (SADCC), or deal with nonstate actors, such as the South African liberation groups that operate primarily from exile havens against the South African regime. The great strength of the volume lies in the superior quality of editing, the fastidious documentation, the skillful juxtaposition of carefully assembled data, and of the pertinent theoretical perspectives and literature.

Although it would be banal to review each separate chapter, it is possible to draw attention to some novel approaches or challenges to conventional wisdom about Southern Africa. Jeffrey Butler writes an exceptionally exciting historical overview of the region and demonstrates very cogently the utility of longitudinal analysis. This in itself has great heuristic value. Larry Bowman, Michael Bratton, and Rukudzo Murapa provide a remarkably lucid demonstration of how to disaggregate the concept of dependence to ascertain the nature, as well as the direction, of change of the asymmetrical pattern of linkage. Their sophisticated analysis furnishes the requisite dynamic element that is often missing from the more flamboyant and deterministic application of this analytical device to third-world situations. Finally, Callaghy draws attention to the fuzziness of the boundaries of the Southern African regional system and points out how Mobutu's Zaire has been included, as well as excluded (in the case of the SADCC), in the turbulent politics of the 11-member subsystem.

RICHARD DALE

Southern Illinois University at Carbondale

Force and Statecraft: Diplomatic Problems of Our Time. By Gordon A. Craig and Alexander L. George. (New York: Oxford University Press, 1983. Pp. xiv + 288. \$19.95, cloth; \$9.95, paper.)

This eloquent book begins with a historical overview of the international system from the

emergence of the great powers in the seventeenth century to the present, proceeds to an analysis of force as an instrument of statecraft for maintaining and managing the modern international system, and concludes with a discussion of the role of ethics in world politics. Written by an eminent historian and political scientist, this work successfully integrates the two modes of thought to create a coherent whole.

A set of critical interrelated assumptions provides the underpinnings of *Force and Statecraft*. First, Craig and George deem a "viable international community" (with an accepted body of law and custom) to be a central prerequisite for minimizing international violence (pp. ix-x). Second, the requirements for this viable international system are (1) an agreement, based on dominant values, among the principal nations about objectives; (2) a structure tuned to the number of interacting states, the scope of the system, and the distribution of power and status among states; and (3) procedures that nations commonly accept for achieving objectives (p. x). Third, the European concert of powers in the nineteenth century is the "experiment" which has come closest to fulfilling these requirements (p. x). Finally, the collapse of this experiment in the twentieth century results from the nineteenth century system's inability to adapt to the rapid and sweeping changes that have created a "diplomatic revolution" in the modern period (p. xi). These assumptions appear quite reasonable, but may mask controversies about placing such a heavy emphasis on the nature of the international system (Kenneth Waltz's "third image") as an explanation of interstate violence and on accelerating change as an explanation of system failure.

The linkage between force and legitimacy—in terms of feasibility, effectiveness, and acceptability—is at the heart of the book. In the context of discussions of force-related aspects of statecraft—including deterrence, coercive diplomacy, crisis management, and war termination—detente stands out as a divergent approach. The American strategy of detente, which granted more political legitimacy to the Soviet Union and to its sphere of influence and enhanced the spirit of reciprocity between the superpowers (pp. 134-136), ultimately failed due to adverse international events (the 1973 Arab-Israeli War and the 1975 Soviet-supported Cuban intervention in Angola) and to the lower public tolerance for the ambiguous and complex detente strategy compared with the powerful "devil-image" Cold War logic (pp. 137-138). If one accepts the authors' somewhat controversial contentions that the Europeans felt detente was a success (p. 147), that the popular threat-based deterrence approach "is often at best a time-buying strategy" (p. 187), and that threats

of force in general are decreasingly efficient in settling superpower conflict (p. 151), then a superpower attempt to return to legitimacy-based strategies like detente rather than force-based strategies might seem appropriate.

Craig and George conclude their book with a telling reminder that, despite rhetoric to the contrary, policymakers often place expediency—what works—over moral purity (p. 277). With the greater difficulty in the modern period of avoiding military moves which communicate escalation toward war (p. 217), this standpoint can be especially dangerous. *Force and Statecraft* is, then, an important guide explaining the presumed obsolescence of both global community and global morality and posing approaches to the seemingly insuperable challenges confronting modern diplomacy.

ROBERT MANDEL

Lewis and Clark College

North/South Relations: Studies in Dependency Reversal. Edited by Charles F. Doran, George Modelski, and Cal Clark. (New York: Praeger Publishers, 1983. Pp. xx + 257. \$27.95.)

Given the enthusiastic and largely uncritical reception of dependency theory by American social scientists in the early 1970s, it is not surprising that a reaction eventually set in. In recent years even the most sympathetic observers of North-South relations have become uncomfortable with the all-or-nothing character of many formulations of dependency theory, with its lack of attention to practical strategies designed to improve third-world countries' standing, and with the vagueness of its proposed alternatives to the existing international order. In addition, many have called attention to the difficulty of applying the more dogmatic versions of dependency theory to the enormous and growing diversity among third-world countries and to the problems that arise from the inclination of many dependency theorists to, in the words of George Lichtheim, "make imperialism rhyme with capitalism" (*Imperialism*, Praeger Publishers, 1971, p. 39).

The contributions to this collection attempt to come to grips with issues such as these, focusing on the possibility of third-world countries' achieving what the authors call "dependency reversal," which "offsets and transforms some of the imputed consequences of dependency" through processes that "may be at work internal to the developing polity and external to it within interstate relations" (p. xi). As is almost inevitable in an enterprise of this kind, the essays are quite diverse, representing a wide range of ideological

and methodological orientations—in this they are only an accurate reflection of the field as a whole. Aside from their diversity of approach, the essays vary considerably in quality: The contributions of several authors, although creditable, are clearly not representative of their best work.

In many ways the most interesting essay is that of Robert Packenham, who offers a spirited defense of what he calls “analytic dependency,” the analysis of “constraints on national units that may occur under any kind of social system (not just capitalism) and which may or may not have [adverse] developmental consequences” (p. 29). The relationship between power asymmetry and exploitation, in Packenham’s view, is an empirical rather than a definitional issue, and he vigorously defends analytic dependency from the challenge of “holistic dependency” in which “empirical data may be used . . . to support the dependency perspective, but never to question its fundamental claims, which are postulated to be true by definition” (p. 32). In particular, Packenham is wary of dependency theorists’ “utopianism” (p. 34) and their vague appeal to world socialism as an alternative to the present world order: “even if one achieves socialism in one country—no easy task, especially given the demanding criteria that can always be invoked for identifying *true* socialism—it is always possible to argue that the fundamental problems remain as long as that one country operates in a capitalist world economy. . . . Without entering into the question of the desirability . . . of [the achievement of world socialism], one can surely say that it is not very feasible in any foreseeable future” (p. 34).

Also valuable is a long essay by Andre Tiano offering a wide range of practical suggestions to third-world policymakers entering into contracts with foreign firms. Tiano tries, usually successfully, to be both reasonable and realistic in offering advice that he expects will offend committed socialists and committed capitalists alike, but which he hopes will be of value in helping third-world countries cope with the imperfect and inevitably compromise-laden world in which they find themselves.

In line with an important theme in the dependency/world systems literature, a number of the essays consider the global system as a whole and speak in terms of centuries rather than years or decades. One such is that of George Modelski, who applies his familiar “long cycle perspective” to the study of dependency reversal. Modelski’s chapter is interesting, although not everyone will be comfortable with a perspective that focuses on “mega-learning processes” that sound suspiciously like the modernization theorists’ notion of diffusion. Another chapter in this genre is Christopher Chase-Dunn’s analysis of “structural

mobility in the capitalist world economy” in which he charts the movement of a number of countries from periphery to semiperiphery and from semiperiphery to core.

The collection also includes several case studies that are useful in that they make an effort to explore relationships—particularly those involving the Soviet Union—that are not typically examined by dependency theorists. Especially valuable is James Ray’s examination of Cuba, which addresses such questions as whether the Cubans have exchanged their traditional dependence on the United States for an even more pervasive dependence on the Soviet Union and whether the Cuban commitment to equality has come at the expense of economic growth. Also interesting is Cal Clark and Donna Bahry’s assessment of ties between the Soviet Union and Eastern Europe, although their conclusion that Eastern European dependence has declined significantly might have been tempered by the more natural comparison of the present situation with the interwar years rather than with the immediate postwar period. Finally, Ladd Hollist offers a useful, if perhaps overly faithful, application of traditional Marxist notion of imperialism to the evolution of Brazilian agriculture, concluding that although the nature of Brazilian agriculture has changed substantially several times over its history, the “sociocultural, sociopolitical, and economic dynamics of the dependency syndrome” have remained much the same over the centuries (p. 181).

In sum, this collection offers a useful, if hardly seminal, contribution toward conceptualizing the position of third-world countries in an international system dominated by countries that are much more powerful and wealthy. As such, it can be recommended to all who are interested in keeping abreast of current debates concerning the pervasive hierarchy that is perhaps the central characteristic of contemporary international politics.

VINCENT A. MAHLER

Loyola University of Chicago

The Caribbean Challenge: U.S. Policy in a Volatile Region. Edited by H. Michael Erisman. (Boulder, Colo.: Westview Press, 1984. Pp. xiii + 208. \$22.50, cloth; \$10.95, paper.)

Central America and the Caribbean have become a growth industry for publishers; *The Caribbean Challenge* is a topical addition to the literature. The nine authors in this collection examine U.S. foreign policy toward that troubled region, particularly during the first years of the Reagan administration. The book is divided into

three sections: the first contains "macroanalyses" or general overviews of U.S. ideological, economic, and military interests in the region. Part 2 presents case studies of tensions in U.S. relations with four Caribbean Basin nations: Cuba, El Salvador, Mexico, and Jamaica. The work concludes with a reprinted essay by Abraham Lowenthal, an influential analyst of U.S. policy in the region.

These essays serve as a comprehensive introduction to recent U.S. policy moves in the Caribbean. Although some essays are more balanced than others, the authors are uniformly critical of the Reagan administration's policies. Of particular concern to these analysts is the reemergence during the last years of the Carter administration and under Reagan of a foreign policy establishment that views the turmoil in the Caribbean region in stark East/West terms rather than as internally generated by poverty and repression. As a result of this return to cold war assumptions, U.S. foreign policy in the area has become increasingly security conscious and stridently anti-Soviet.

Washington's perception of events in the Caribbean Basin primarily in East/West terms has added stress to already taut relations in the case of several countries. All of the authors agree that Cuba is seen by the Reagan administration as "the Soviet empire's westernmost strategic outpost" (p. 99); but from the Caribbean perspective, Cuba is gaining acceptability as a legitimate actor with its own foreign policy agenda. The contributors are somewhat less in accord on how the United States should proceed with Havana. Josefina CINTRON Tiryakian calls for establishing close economic ties, whereas Juan del Aguila cautions that ending the U.S. trade embargo may be a step in the right direction, but "to suppose . . . [it] will make Havana behave is inconsistent with practical realities" (p. 107). In John Booth's analysis of El Salvador, Bruce Bagley's discussion of Mexico, and Jacqueline Braveboy-Wagner's chapter on Jamaica, Washington is also seen as misguided in assuming that all regional problems are inextricably tied to superpower rivalry. Furthermore, because of Washington's heightened security concerns, the Reagan response to regional economic problems is inappropriate. The chapters by Kenneth Boodhoo, Braveboy-Wagner, and Lowenthal give detailed critiques of the Caribbean Basin Initiative unveiled in February, 1982. They argue convincingly that the Initiative reflects primarily the present administration's "interest in military security, political loyalty, and advantages for U.S. firms rather than the region's long-term development" (p. 193).

While *The Caribbean Challenge* will provide a useful introduction to U.S. policy in the Caribbean, this recommendation comes with some

caveats. First, the volume will have a brief shelf life, because it focuses narrowly on the early years of the Reagan administration. Second, the volume has no concluding essay to summarize and expand on contributors' insights; the Lowenthal essay is excellent but does not serve this function. A final concern is for the use of the construct, "Caribbean Basin." The concept has been created by U.S. military planners to meet American strategic needs. This notion, though, seems artificial to the bulk of regional inhabitants—the people of the Caribbean archipelago and Central America who have dissimilar historical and cultural traditions. The artificiality of the "Caribbean Basin" was evident during the Falklands/Malvinas crisis, for example, when the English-speaking Caribbean supported British claims to the islands, while the Hispanic nations of the region backed Argentina. Some analysts, including *The Caribbean Challenge's* editor, believe the Basin construct is useful since the insular Caribbean is becoming more "Central Americanized" or open to U.S. domination and intervention. Evolving relations between the U.S. and the nations of the region will resolve the debate.

SHERRIE L. BAVER

The City College of New York

The Middle East since Camp David. Edited by Robert O. Freedman. (Boulder, Colo.: Westview Press, 1984. Pp. xv + 263. \$28.50, cloth; \$11.95, paper.)

It is in the nature of annual surveys of recent trends and events in any region of the world to do little more than rehash old headlines and perhaps provide a chronological listing useful for future reference. The reader has little right to expect fresh viewpoints and profound analysis. By and large, this collection of eight papers originally presented at a May, 1982 conference sponsored by Baltimore Hebrew College (and later updated to include the Israeli invasion of Lebanon) conforms to these expectations. The contributions, heralded in the Introduction as providing "an encompassing analysis of the major developments in Middle Eastern politics from the Camp David agreements of 1978 until the Israeli invasion of Lebanon of 1982" (p. 5), are not badly done, but neither are they replete with new and exciting insights.

The volume is comprehensive in organization, even if the essays vary considerably in quality. The first two (of three) parts concentrate on superpower and regional perspectives. Freedman's lengthy survey of Soviet policy leads off the book and is followed by Barry Rubin's

summary of the U.S. role during this period. In the second section, Shireen T. Hunter and Robert E. Hunter look at trends in the Arab world, and Robert G. Darius examines recent developments in Iran and their impact on that country's foreign policy. The third part (taking up one-half of the book) consists of analyses of individual actors: John F. Devlin on Syria, Don Peretz on Israel, Louis J. Cantori on Egypt, and Aaron David Miller on the PLO. A brief epilogue by Freedman summarizes developments through October, 1983.

A lot happened in these four years, but the Middle East seems to have survived, despite its much-mentioned "instability" and "volatility." Neither the Camp David approach nor the Iranian Revolution has transformed the Middle East as it once appeared they might. In Lebanon, the fifth Arab-Israeli war left Israel with more occupied territory and deeper internal divisions, underscored the Arab world's inability to act in unison, and added another layer to Lebanon's burden. Despite the initial fears, the Iran-Iraq War did not result in catastrophe but remains locked in a weary stalemate. Far from closing in on the Gulf's oil, the Soviet Union has seen its influence waver and decline. And the United States, despite the setbacks and disappointments in its policies toward the region, is no worse off there than before.

This survey may prove useful as a means of quick review. It may also be usable as a supplementary textbook for courses on the Middle East or international relations, although its advantage in breadth of coverage is offset by the short time-period it considers and the lack of sufficient background to adequately explain events and place them in the proper context.

J. E. PETERSON

Washington, D.C.

Risks of Unintentional Nuclear War. By Daniel Frei and Christian Catrina. (Totowa, N.J.: Allanheld, Osmun, 1983. Pp. xxxi + 255. \$10.95, paper.)

Nuclear War and Nuclear Peace. By Gerald Segal et al. (New York: St. Martin's Press, 1983. Pp. xii + 162. \$25.00.)

In *Risks of Unintentional Nuclear War*, a study commissioned by UNITAR's Institute for Disarmament Research, Daniel Frei evaluates the chances that nuclear war will occur unintentionally, not from rationally premeditated calculations of strategic or political benefit by adversaries in conflict. He argues that accidental nuclear war is unlikely given the redundancy of modern nuclear command, control and com-

munications systems, and leaders' caution. But unintentional war would be the product of decisions "based on false assumptions, i.e. on misjudgment or miscalculation" by legitimate authorities (p. ix). Human misunderstanding connected with short decision times leads to crisis instability. War could begin at a low level of armed conflict, and miscalculation, paranoia, the imperatives of vulnerable weapons systems, decision-maker stress, or organizational foul-ups could lead unintentionally to nuclear escalation.

The book does not contain new arguments or research. It instructively organizes the observations and conclusions of the relevant literatures of strategic doctrine, game theory, organizational process, cognition, and social psychology that bear on crisis stability. At the end of each chapter, Frei draws conclusions regarding the contributions to the risk of unintentional nuclear war of the factors discussed, and the factors that mitigate such factors. His final conclusions emphasize the most worrisome risks: weapons systems and doctrines that lead to short decision times; doctrines that create the belief by one nuclear power that the other is seeking nuclear superiority, or lacks commitment to its own and its allies' defense; decision processes that exacerbate stress and reduce information availability; and unwise patterns of security commitments by the superpowers. Frei sets out 32 recommendations to mitigate the risks, including some novel ideas. He suggests that the superpowers negotiate a "code of conduct" for their relations in third-world environments and for crisis management. He proposes that negotiations should be undertaken to establish new means of arms control verification, perhaps based on the experience of the International Atomic Energy Agency's nuclear safeguards system.

The Frei book, *Risks of Unintentional Nuclear War*, is a comprehensive discussion, based on familiar but sound materials, of crisis instability in the nuclear strategic context. Its methodical combination of technological and human factors is its innovative contribution to the nuclear literature. Its synthesis of important literatures is excellent. The final recommendations are creative, important, and sensible. It would be useful to have a subject index, but the text is copiously referenced and the sources are listed in an extensive bibliography.

The authors of *Nuclear War and Nuclear Peace* have struck an elegant blow for the "radical centre" against attacks by both right and left on Western nuclear strategic thinking, NATO, and arms control efforts. The primary target of this well-integrated set of essays is the Campaign for Nuclear Disarmament and its European and U.S. analogues that variously support denucleariza-

tion, nuclear freezes, nuclear free zones, decoupling the United States from NATO, and unilateral disarmament. The authors argue that because nuclear weapons technology cannot be "unlearned" and that it is employed in support of political objectives, realism requires involvement in the messy business of making the nuclear world a comparatively stable one. Nuclear weapons are not the source of East-West discord, but are central to the military confrontation that emerges from it.

The authors establish the morality of a sensible "centre" policy by discrediting the moral arguments of the extremes. The Right claims that new technologies such as "star wars" could enable the West to fight a war against weapons, not civilians. The Left argues that nuclear weapons are immoral because the threat of their use is morally tantamount to their use. The authors argue that it is more moral to pursue strategies that effectively deter the use of nuclear weapons than it is to follow either the Right or the Left's potentially destabilizing prescriptions.

Segal points out the basic asymmetry of U.S. and Soviet nuclear doctrines—the United States basing deterrence on assured destruction while the Soviets stake their survival on the deterrent effect of a credible war-fighting capability. But he argues that both can be made more secure through arms control and the deployment of appropriate, less vulnerable, weapons. Edwina Moreton argues that arms control is useful regardless of the state of U.S.-Soviet relations, and for arms control agreements that lead to greater strategic stability. She calls for reexamining the sanctity of the United States strategic triad and considering elimination of land-based ICBMs. Lawrence Freedman reminds us that emplacement of Pershings and GLCMs in Europe was a response to European fears of the United States decoupling from NATO, and maintains that their deployment is preferable to decoupling or to maintaining existing levels of battlefield tactical nuclear weapons as symbols of U.S. commitment. John Baylis argues, in a more narrowly focused essay, that a British nuclear deterrent is preferable to unilateral disarmament or precipitous moves in the direction of either a consolidated European nuclear force or total reliance on conventional weapons.

Nuclear War and Nuclear Peace is a stimulating and thoughtful work written in an engaging and succinct style. The book's "radical centre" position could serve admirably as a basis for late 1980s strategic and arms control policy.

BENJAMIN N. SCHIFF

Oberlin College

The Voice of the Poor. By John Kenneth Galbraith. (Cambridge, Mass.: Harvard University Press, 1983. Pp. v + 88. \$8.95.)

If one keeps in mind that this book is based on a set of lectures and is not meant to be a complete argument, it can be read with great profit. Because Galbraith makes a number of major implicit assumptions, the book must be read as a starting point, not as a definitive statement.

Galbraith's wholly admirable purpose "is to suggest some of the things which . . . rich countries should be hearing from the poor" (p. 3). Galbraith's first major assumption is that all poor countries would want to say the same things, at least approximately, to the rich countries. Although this is clearly not true, it is benign for the purposes for which it is used. However, a second assumption is less benign: Any given poor country speaks with one voice, and any internal conflicts are, for this purpose, less important than are the commonalities within the poor country. Current events in Lebanon, El Salvador, and elsewhere certainly seem to belie this. For example, one can certainly imagine that the poor members of a poor country, but not the rich members, might like to say to any rich country: Why should we take your advice when there are numerous poor people in your country? Galbraith appears to have fallen into a common oversimplification: In poor areas almost all are poor, and in non-poor areas virtually none are poor.

This book is composed of four main chapters. In the first, Galbraith wants to remind everyone that a country's historical situation places constraints on what it can do. Galbraith claims, correctly, that both the United States and the USSR ignore this, and also ignore their own histories. Thus, these two countries give advice and push programs and practices that are bound to have unintended consequences as well as failing to achieve their explicitly intended consequences. Although both correct and stated very well, Galbraith ignores two points and makes an implicit assumption about a third: Today's poor countries are in a different situation than today's rich ever were; there are some rich countries today, and it may be possible to learn from them. Do the United States and the USSR actually intend beneficent consequences for the poor countries when they give aid and advice? Certainly this can at least sometimes be doubted.

Galbraith generally appears to mean economic growth by "development," but it may be more appropriate to distinguish development from growth according to who benefits, with development implying a more equitable distribution of income.

Chapter 3, the second major chapter, deals with the modern form of imperialism: influence. Galbraith argues that this is bound to fail because "the will to national independence is the most powerful force in our time" (p. 36) or, "the unbounded and universal determination of people everywhere to govern themselves" (p. 42). This and the preceding chapter would have benefited from less independence. The obvious question is, What is a nation? A historical look at the results of the old version of imperialism convinces one that many nations exist at least partly because of the old imperialism. For example, in Lebanon, political offices are guaranteed to certain religious groups, while others are frozen out. This, of course, does not excuse the United States and the USSR for their acts or intentions, but it does make answers more complicated.

Chapter 4 concerns the military relations between the rich and poor countries. Galbraith is correct that these relations hurt the poor countries in several ways.

In his final chapter, Galbraith switches perspectives to discuss the domestic policies of the rich countries. Again, he is correct that reliance on monetary policies in the United States results in high interest rates that draw money to the United States from other countries. The results of this migration are not going to be helpful to most residents of poor countries, although those who are already rich may become even richer.

Throughout, Galbraith writes with grace and wit and in a manner likely to provoke thought rather than stifle it. The result is a book well worth reading even by those mainly concerned with domestic politics and policies.

RICHARD GOLDSTEIN

Brighton, Massachusetts

The Pharmaceutical Industry and Dependency in the Third World. By Gary Gereffi. (Princeton, N.J.: Princeton University Press, 1983. Pp. 291. \$25.00, cloth; \$9.95, paper.)

Gereffi has labored long and intensely to gather data on the operations of pharmaceutical companies in Mexico, South America, and even Sri Lanka. He has been able to transpose into readable English the technical jargon of medicine manufacturing so that the details about tranquilizers, sedatives, and analgesics and their significance to the world market become readily understandable.

Because the developing world's population is often not within walking distance of health clinics nor has adequate purchasing power to supplement welfare medicine with their own purchases, an

analytical look at third-world pharmaceutical manufacturing is indeed useful and helpful to those trying to gauge that facet of national development.

Gereffi takes several legitimate cases and somewhat documents his hoped for political indictment, however from that he draws generalizations that do not hold globally. He strives to fit his field data and analyses into dependency theory; technology transfer benefits are not cited.

Modernization theory became established in the social sciences after World War II as third-world nationalist movements emerged. In the 1950s and 1960s, such studies compared the validated norms of the market place among Western democracies and underdeveloped nations. In the 1960s and 1970s, a new generation of social scientists promoted a rival theory of dependency, an amalgam of Marxist and other-variety socialist perceptions. With a framework traced back to Lenin's works, by the 1970s a new network of academicians had out-published the modernization theorists, not in quality but in quantity.

Dependency theory contends there is always a drain of economic surpluses from the less-developed nations to their foreign investors. Such a position ignores postwar expropriations. More important, dependency theorists continue to ignore the return of foreign profits in elaborate infrastructures that nationalistic regimes usually then nationalize, and the huge foreign debts to Western democracies that will never be repaid, whether they are owed to private banks, governments, or combinations of capitalist creditors.

In examining the Mexican industry that manufactures steroid hormone medicines, Gereffi focuses on Syntex, beginning in the 1950s when there was a corporate debate about whether Syntex should fundamentally produce primarily intermediate steroids or whether it should enter the finished products markets. Corporate leaders decided to market some finished products made in Mexico. Gereffi asserts that the decision had something to do with U.S. Senate antitrust hearings, but the company's operations were located entirely in Mexico at the time (p. 109). It was not until 1959 that Syntex moved to Palo Alto, California.

On page 110, Gereffi charges that, to avoid taxes, Syntex produced finished drugs in its plant in Puerto Rico. However, Puerto Rico is part of the United States. The Commonwealth gives tax advantages to encourage mainland industries to open branches on the island, but semifinished drugs from Mexico that received final processing were and are subject to taxation on the added value.

Since 1973, Mexico has succeeded in reducing its annual population growth from the disastrous

3.6% in 1976 to 2.9% in 1983. More than 7,000 Centers for Family Planning distribute free contraceptives to Mexican women. In his eagerness to indict multinational corporations, Gereffi does not mention the expense these foreign firms incurred in Mexico in training Mexican chemists and the expenses involved in pioneering the manufacture of the pill in Mexico by Syntex, Ortho, and Parke-Davis, and in phasing out sales to Mexican concerns. That doesn't fit the dependency theory, but it certainly affirms the older developmental theory.

Nevertheless, a careful reading of this volume will yield numerous useful capsule histories of the political advantages and disadvantages that beset pharmaceutical concerns in Latin America. The basic scholarship on government-corporation relationships is excellent. Some of the generalizations drawn to fit theory, however, deserve a caveat.

MARVIN ALISKY

Arizona State University

The United States and Mexico: Patterns of Influence. By George W. Grayson. (New York: Praeger Publishers, 1984. Pp. xvii + 215. \$27.95, cloth; \$13.95, paper.)

This book is part of the series "Studies of Influence in International Relations" edited by Alvin Z. Rubenstein. Influence is "demonstrated when either country affects through nonmilitary means, directly or indirectly, the behavior of the other in a manner designed to achieve a policy advantage" (p. 2). Influence is a derivative of power, but influence is situational and short-lived, whereas power is latent, continuing, and relative. Grayson never makes the difference clear, and it is critical to an understanding of U.S.-Mexico relations because power determines the nature of influence. Grayson contributes to this analytical confusion in the introductory chapter when he discusses seven problems in analyzing influence patterns in U.S.-Mexico relations that make such analysis very difficult. And in the Conclusion he adds, "Smoke from dozens of sources obscures the view of influence patterns in U.S.-Mexican relations" (p. 193). In truth, there is little attempt to be systematic in applying the analytical framework. A list of six questions (p. 6) to be given special attention to determine why a given policy outcome occurred is hardly referred to in the text. The text itself treats the historical relationship between the two countries, including chapters on the Echeverria and Lopez Portillo years, with the chief focus on oil and gas, then discusses the role of Mexico in the Central American crisis, the failure

of Mexico to join GATT, the continuing problem of immigration, and the economic crisis of 1982. There seems to be a conscious effort to stay away from two concepts used by many analysts in approaching bilateral relations, dependence, and interdependence. Grayson mentions dependence, but there seems to be an aversion to employing the concept of interdependence, which would be most useful in the case of the debt crisis of 1982. His view is that the debt crisis increased Mexican dependence, but a more realistic view would be that it highlighted the interdependence between the two countries. It should be added that several bilateral problems, such as maritime, border, and foreign investment issues are not discussed. Although membership in GATT is mentioned, very little is said about other trade issues that could well be the central bilateral issues of the 1980s. Grayson begins the book with the statement, "The United States relationship to Mexico is more critical to its vital interests than any other bilateral ties, with the possible exception of those with the Soviet Union" (p. xi). What seems to justify this incredible assumption is a sense of impending doom for Mexico and its relations with the United States; the book is replete with this sense of catastrophe about to happen, for example, Grayson concludes that a peace accord that permitted thousands of anti-Sandinistas to resettle in the United States could alter the outlook for effective immigration reform (p. 166). He is no sympathizer with illegal immigration; in discussing the Simpson-Mazzoli bill, he adds, "Chicano leaders, encouraged by Mexican politicians, have even excoriated this modest congressional initiative on the grounds that it will accentuate civil rights abuses and produce a laterday Third Reich" (p. 166)—hardly a fair assessment of genuine concern for civil rights on the part of Hispanics. In conclusion, primarily because of its fuzzy analytical framework, this book is not a major contribution to the literature on U.S.-Mexico relations.

C. RICHARD BATH

University of Texas at El Paso

The Reluctant Supplier: U.S. Decisionmaking for Arms Sales. By Paul Y. Hammond et al. (Cambridge, Mass.: Oelgeschlager, Gunn & Hain, 1983. Pp. xv + 310. \$27.50.)

The Reluctant Supplier furnishes a very useful descriptive account of selected aspects of post-World War II arms transfers. The authors provide an excellent summary of the role of arms transfers in U.S. foreign policy and the changes successive administrations have introduced. They give a detailed account of the institutional decision-

making process for U.S. arms transfers, including the reforms implemented under President Carter and largely continued by the Reagan administration.

Hammond et al. use extensive interviews with executive branch officials along with primary material to develop a case study of President Carter's failed Conventional Arms Transfer Talks. Interviews with relevant personnel were also used in a study of the 1970s' reduction in the roles and numbers of U.S. military advisors. Finally, the authors present a valuable summary of regional arms transfer data, reporting both agreements and deliveries in constant dollar measures whenever possible.

The authors carefully demolish the claims that arms transfers are accelerating in volume, with the United States leading the charge. Hammon et al. point to factors that promote the demand and supply of arms transfers. However, they emphasize that neither demand nor supply are limitless. Budgetary and foreign exchange problems among others constrain demand, especially among the poorer third-world nations. Supplier states are constrained by the requirements of their own militaries. Additionally, the United States has a history of legislative resistance to arms transfers.

Hammond et al. employ constant dollar measures to show that worldwide arms demand has risen, whereas U.S. transfers, counting grants and government and commercial sales, have remained stable. This conclusion may overstate stability, because the government sources the authors use probably understate commercial sales. Nevertheless, these findings continue the work of other scholars in refuting the "merchants of death" perspective so often found in popular and professional literature. Hammond et al. challenge assertions that world arms transfers are escalating without limit while the United States is massively increasing its own transfers.

There is some imprecision contained in the authors' use of aggregate measures to indicate a rise in worldwide demand for arms. The weapons being bought since the 1960s are more sophisticated and expensive than before. Major arms suppliers are transferring much state of the art technology. An increase in procurement outlays by a government may reflect efforts to replace older arms and upgrade aging inventories, rather than an increase in demand. Such increases may also reflect rising GNPs, resulting in larger state budgets and thus more funds for procurement. In general, this seems to be true of most developing countries as their military budgets increased only slightly as a percentage of GNP between 1967 and 1976, a period that corresponds to the arms demand analysis in *The Reluctant Supplier*.

In general, Hammond et al. support using arms transfers to pursue American policy goals. They are critical of the mid-1970s' reductions in the numbers of U.S. military advisors and the limits put on the roles of military missions. They favor using military advisory groups as advisors and "in country" spokesmen for U.S. policy. They also find fault with the Carter administration's arms control efforts. They seem more comfortable with the Reagan administration's approach to arms transfers, although they are mindful of the uncertain gains such efforts can bring. Indeed, arms transfers can have many contradictory impacts on relations between states. However, the authors contend that great powers cannot long abstain from using such a widely desired commodity to gain influence and pursue their own goals. They provide an eloquent statement of a mainstream perspective on arms transfers. They take most established U.S. foreign policy goals as givens and focus on modes of realizing them.

This mainstream orientation leads to little emphasis being given either to the Third World or the implications that new, highly lethal weapons have for levels of destruction and regional power asymmetries. New weapons also require even greater infrastructure investments, skilled labor, and supporting industries simply for maintenance, to say nothing of coproduction. These requirements hardly match the economic development strategies most appropriate to third-world resources and social needs. Instead, a "dependent militarism" evolves with developing nations ever more skilled labor and technology. The authors give little attention to the effects arms transfers have on third-world populations and economies.

Finally, Hammond et al. single out Andrew Pierre for an unfair and hyperbolic attack. The authors imply that in his *The Global Politics of Arms Sales* (Princeton University Press, 1981) Pierre uses only current dollar figures. This is not supported by the pages they cite. The attack is aimed at Dr. Pierre's analysis, but comes closer to being an *ad hominem*.

M. STEPHEN PENDLETON

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The Bar Kokhba Syndrome: Risk and Realism in International Politics. By Yehoshafat Harkabi. Edited by David Altshuler. Translated by Max D. Ticktin. (Chappaqua, N.Y.: Rossel Books, 1983. Pp. xvii + 206. \$15.95.)

Between 132 and 135 A.D., the Jews of Judea led by Bar Kokhba revolted against the occupying Roman authorities. Although it was brutally crushed by the militarily superior Romans, Bar

Kokhba's rebellion has been mythologized in contemporary Israel as a symbol of heroism worthy of emulation. In his provocative and enlightening work, Yehoshafat Harkabi debunks this myth and analyzes the implications for international politics in general, and for Israeli foreign policy in particular, of what he terms the "Bar Kokhba Syndrome"—the blind glorification of rebelliousness and heroism detached of responsibility for, and oblivious to, their outcomes.

Challenging the popular view that the uprising was successful, Harkabi argues convincingly that the rebellion, judged by its ultimate consequences, was an unprecedented disaster because it caused the death of 580,000 Jewish soldiers, led to the exile of the Judean inhabitants, and transformed the Jewish people from active subjects to passive objects of history. The revolt ended in catastrophe, according to Harkabi, because its leaders failed to anticipate the inevitable outcome, did not appreciate the limited nature of their resources, and refused to consider more realistic alternatives that might have averted national suicide.

For Harkabi, the rebellion epitomizes the height of political irresponsibility because "it is never in the national interest to behave suicidally" (p. 64). At the same time, this tragic episode enables him to distill important lessons about the requirements of rational choice and the perils of risk-taking. First and foremost among these is the need to pursue realism in political action. Realism consists of several elements: envisaging the consequences of political decisions, recognizing that decisions often entail a choice between bad and worse options rather than between good and evil outcomes, realizing that dreadful consequences may result from well-intentioned policies, refraining from provocation for which one's adversary may have only one devastating response, and avoiding actions whose success depends entirely on optimal conditions.

Harkabi laments the failure of Israel's political leadership, particularly since 1967, to draw these lessons, decries the tendency to exalt the Bar Kokhba uprising as a heroic episode, and calls for a national self-reckoning based on a more accurate reading of history. He contends that recent Israeli governments have been intoxicated by the Bar Kokhba Syndrome by manifesting a proclivity to excessive reliance on force, downplaying the severe constraints of national resources, and obfuscating distinctions between ephemeral tactical victories and enduring success at the strategic level. Unrealism is also rampant among Israeli doves, who assume erroneously that the PLO and the Arab states have already reconciled themselves to Israel's existence, and it is prevalent among hawks, who refuse to make the necessary ter-

ritorial concessions to moderate the Arab-Israeli conflict. Harkabi concludes that a realistic policy requires Israel to eschew annexation, withdraw from the occupied areas, and acknowledge Palestinian rights to national self-determination, preferably in a confederal arrangement with Jordan.

Although Harkabi deserves praise for an incisive and trenchant analysis of risk-taking and its implications for contemporary international politics, his work is not without shortcomings. In recommending the "Jordanian option" as the optimal solution to the Palestinian question, Harkabi appears to have forgotten his own caveat that lessons drawn from history are more helpful in identifying actions that must be avoided than they are as prescriptions for specific policy outcomes. In addition, the prose is unduly repetitive, and the final chapters are somewhat disjointed. The book is also inundated with typographical errors.

Nevertheless, Harkabi's brilliant exposition of the Bar Kokhba Syndrome is an intellectual *tour de force* and provides original insight on responsible statesmanship and rational choice in both national and international political realms.

MICHAEL RUBNER

Michigan State University

Armed Forces and the Welfare Societies: Challenges in the 1980s: Britain, the Netherlands, Germany, Sweden and the United States. Edited by Gwyn Harries-Jenkins. (New York: St. Martin's Press, 1983. Pp. xiv + 218. \$29.00.)

This is a defense scholars' salvo in the guns-versus-butter debate now annotating the transition from the postwar era to the emerging prewar world. The book's contributors are a half-military, half-civilian, almost wholly European research team sponsored by Britain's International Institute for Strategic Studies. Their message is that the West has reached a critical threshold in civil-military affairs. This crisis results from the strain on peace-lulled materialistic societies because of escalating military materiel and personnel costs imposed by technology and the welfare state. They warn that our thoroughly demobilized welfare states face radical changes in their national defense establishments unless threat perceptions soon rise.

An editor's introduction to pose the issues is followed by competent case studies of British scrimping, Dutch experimentation with military democracy, German conscription problems, the Swedish model of universal military training but

no standing army, and American efforts to maintain a professional military establishment. A postscript spells out lessons from the case studies. The main problems are: (1) the technology-sophistication factor is forcing smaller states to stop weapons system production; (2) the welfare state drive toward equality combines with low threat perception levels to make conscription decreasingly viable; (3) high government and industry pay levels are thwarting military efforts to attract highly qualified manpower; (4) antimilitary attitudes and/or sheer indifference to defense requirements are creating tremendous pressures for change of traditional military structures; and so, (5) projecting expected costs of traditional establishments yields a head-on political collision between national concerns for security and for social justice within a decade.

For these analysts demographic projections are also ominous: The trend of steady and drastic decline throughout the West in the traditional male 18 to 24 age cohort from which to draw essential manpower will continue until the mid-1990s. This trend melds with the technological and other cost factors to reduce each state's ability to maintain strong forces in being. But because technical progress rapidly increases real weapons hardware costs, the quandary comes full circle. The automated battlefield is forcing each West European state to specialize; none can long maintain independent industrial capabilities for land, sea, and air weapons development. The United States may soon consolidate its monopoly of aerospace and high technology weapons. As there arises a new generation unacquainted with war, increasingly Europeans face the prospect of becoming military client states of America and of drifting toward neutralism at the same time. These tendencies combine with welfare state values leading to military unionism, increased soldier pay, regularized working hours and conditions, and lower social status for officers, and so threaten to turn the military profession into just another occupation or—much worse—into a potential scapegoat for hostile public opinion.

Until recently the military establishments enjoyed public indifference and a "splendid isolation" from societal influences, allowing each to operate as a "total institution" (pp. 74, 77). Because this favored status no longer applies, and because in Europe's stagnant economies only hard political decisions can prevent the welfare states' crumbling, challenges to the armed forces' political legitimacy are at hand. Many Europeans simply believe their armed forces cannot provide national security. The authors conclude that it may be impossible to maintain European armed forces in their traditional form and size, and that morale problems may become chronically debili-

tating—however the "men versus technology" debate is resolved. Predictions here are sketchy.

The U.S. chapter shows a marked contrast. America's recently renewed sense of both danger and purpose may reinforce a "fundamental cyclical 'swing' in the public mood back toward another period of extroversion and interventionism abroad" (p. 184). Thus Alan Ned Sabrosky correctly finds America at a turning point of the Klingberg Cycle. This is a 50-year alternation of mood between introversion and extroversion discovered by Frank L. Klingberg and described in his *Cyclical Trends in American Foreign Policy Moods* (University Press of America, 1983). Sabrosky asserts: "American commitments abroad are virtually certain to be maintained as part of a more fundamental reassertion of American activism in foreign affairs" (p. 192). The European contributors do not yet understand this cycle, but it should help solve the problem of low threat perception.

JON ALEXANDER

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Strategic Military Surprise: Incentives and Opportunities. By Klaus Knorr and Patrick Morgan. (New Brunswick, N.J.: Transaction Books, 1983. Pp. vi + 265. \$14.95, paper.)

This book represents an introductory study of a subject that has not received extensive scholarly attention. Using a case study approach, the authors and their contributors, Michael Doyle, Michael Handel, and Richard Betts, have identified some of the key factors that characterize strategic military surprise. The effort is avowedly heuristic, but it also has great substantive value. Although the conclusions are necessarily tentative and qualified, they are reasonable and thought-provoking. This book is a significant contribution in a relatively new field of strategic studies.

The 20 case studies are presented in four chapters. The time frame covers 107 years, from the Austro-Prussian War of 1866 through the Yom Kippur War of 1973. The cases represent a wide range of contexts, including geographical variation (Europe, the Far East, Latin America, and the Middle East) and relative capabilities of attackers and victims (equal and unequal). In all cases, the attacker's "surprise" was the result of at least one of three factors: unexpected initiation of military hostilities (e.g., by Germany in 1914 and Japan in 1941); unexpected extension of military hostilities in the course of an existing conflict (e.g., in the cases of Inchon and Tet); and/or use of an unexpected or new mode of warfare (e.g., the German Blitzkrieg and the 1945 Soviet inva-

sion of Manchuria). A "strategic" attack is defined as one which attempts to "inflict a striking defeat that sharply alters the military situation and possibly determines the outcome of the conflict" (p. 1).

Given these somewhat ambiguous definitions, the case studies, which constitute much of the book, do fit the authors' criteria of strategic surprise. The intriguing question is why any given attack was a surprise, and the answers in this book range from the secrecy of the attacker's plans (as in the case of the use of the atomic bomb against Japan) to the more general and recurrent theme of misestimates, which is stressed in cases as diverse as the Franco-Prussian War and the battle of Dienbienphu.

Despite the value of this book, it has several weaknesses. The authors and contributors do not adopt a common framework, even though one is presented in the introductory chapter. This results in an analysis that at times lacks focus and direction. An appendix cataloging the various hypotheses would have been extremely helpful. Another problem is the lack of any rationale for the selection of cases. For instance, why was the 1940 German attack on Norway included while more obvious cases like Midway, D-Day, and the 1975 North Vietnamese Army offensive were excluded? A third problem is an occasional lack of conceptual clarity. For example, the distinction between an unanticipated attack and an attack that is anticipated as being unlikely is not regarded as significant in terms of "surprise," whereas such a distinction is critically important in decision-making analysis. Finally, there are enough typographical errors to make reading the book somewhat disconcerting.

Despite these problems, this is a useful and provocative book. The last three chapters are particularly important. They summarize the case studies by focusing, respectively, on the attacker's incentive structure, the elements that constitute the opportunity for strategic surprise, and some general prescriptions for policymakers. The pessimistic conclusion is that the incentives and opportunities for strategic surprise will continue to make it a viable and generally successful policy option, despite technological advances in various warning systems and conscious decision-making efforts to assess an adversary's capabilities and intentions.

STAFFORD T. THOMAS

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Superpower at Sea: U.S. Ocean Policy. By Finn Laursen. (New York: Praeger Publishers, 1983. Pp. xiii + 210. \$24.95.)

On Saturday, March 3, 1981, a two-sentence notice was tacked onto the bulletin board in the State Department newsroom. This modest note fell like a bomb on the worlds of diplomacy, international law, and international relations; it announced that the United States would seek to ensure that the Tenth Session of the Third United Nations Conference on the Law of the Sea—due to open only two days thence—would not, as scheduled, be its last by preventing conclusion and adoption of a comprehensive convention on the Law of the Sea. Furthermore, virtually the entire U.S. delegation to the Conference, capable and experienced people, were summarily dismissed, and a far smaller United States delegation (headed by an individual of no visible qualifications) was instructed not to engage in any negotiations whatever pending completion of a thorough review of the draft treaty then under consideration. Thus ended, abruptly and unceremoniously, more than a decade of constructive American efforts to achieve through multilateral negotiations a legal regime designed to insure peaceful and orderly use of more than 70% of the earth's surface.

Finn Laursen, a Danish political scientist, was present at several sessions of UNCLOS III and has written previous analyses of U.S. policy and policymaking in connection with the Law of the Sea. This book covers the evolution of this policy from the late 1960s through the signing of the United Nations Convention on the Law of the Sea at Montego Bay, Jamaica, on December 10, 1982. More than a chronological description of the evolution of American policy on the numerous complex issues of the Law of the Sea, the account is decidedly analytical.

In an effort to try to explain the policies described, the study applies four analytical models, namely, a modernized version of classical realism (statist goals), international independence, bureaucratic politics, and domestic politics. All four models can help to describe and explain some aspects of U.S. ocean policy in this period, but domestic politics is found to have the greatest explanatory power. It explained the passage of the Fishery Conservation and Management Act of 1976, and the Reagan shift away from multilateralism in 1981 and 1982. (p. ix)

The book is organized into nine chapters, each followed by numerous notes, primarily citations of sources. The text is followed by a "selected bibliography" that includes a list of publications of the Center of International Studies of Prince-

ton University, under whose auspices Laursen wrote this book. The chapter notes and the selected bibliography together form a comprehensive and useful collection of published materials for further reference. The index is also useful.

Laursen begins with an overview of the problem in which he summarizes developments in both the Law of the Sea and U.S. policy in relation to it. Then he explains the analytical perspectives and hypotheses to be applied to the problem. The next five chapters are straightforward descriptions of the major components of U.S. ocean policy through the Carter administration—the politics of security, offshore petroleum rights, fishing, and deep seabed mining—and of the Draft Convention and the Reagan review of it. Chapter 8 applies the analytical tools described earlier to these various conflicting interests within the United States government and society, and the final, very brief, chapter contains concluding remarks.

Laursen did not have the kind of access to, and experience with, the inner workings of the United States government as did, for example, Ann Hollick, Robert E. Osgood, and John Norton Moore, who all have written extensively about U.S. ocean policy, but Laursen has used a wide range of published materials including, wisely, some of the materials produced by nongovernmental organizations (NGOs) represented at UNCLOS III. I am impressed with the accuracy of Laursen's descriptions of events and of American decision making, which undoubtedly results from his attendance at several sessions of the Conference and his association with NGOs, as well as his ability and diligent research.

This book is a superb case study of foreign policy decision making and as such would be a useful supplement to textbooks in a variety of foreign policy and international relations courses, but because of its specialized nature has limited value as a text itself. It is balanced, both ideologically and between fact and theory, but it is quite clear in its conclusion that the Reagan policy regarding the Law of the Sea is misguided and harmful to the long-term best interests of the United States. It ends with the hope that "maybe a future administration in Washington will give up confrontation and return to law and diplomacy" (p. 181).

MARTIN IRA GLASSNER

Southern Connecticut State University

War in the Modern Great Power System, 1495-1975. By Jack S. Levy. (Lexington: University Press of Kentucky. Pp. xiv + 215. \$24.00.)

This volume is the integration of Levy's work on war, beginning with his award winning dissertation and carried admirably forward in several important, widely cited journal articles (some material from which is reprinted in the volume). The first chapter posits that "great power" wars are different than wars in which lesser powers are the principal actors; as a consequence, he claims, they must be analyzed separately. Moreover, Levy argues the modern power system began neither in 1815 as many political scientists assume, nor following the Treaty of Westphalia in 1648 as many historians assert, but rather emerged at the end of the fifteenth century following the signing of the Treaty of Venice in 1495. A "Great Power" is defined to have a high level of relative military capabilities and a conception of their interests as global rather than local; to pursue their perceived interests more aggressively, often using military threats and force; to be recognized by other great powers; and to often possess certain perquisites in international organizations (veto power, for one). Levy's actual list of great powers is catholic. What is especially important here is that Levy is not simply extending a set of analyses back in time. He is making a theoretical argument that great powers are different, and that analyses that fail to recognize that fact face potential validity problems. This assertion itself is not empirically examined in the book.

Levy has done yeoman work in generating a data set from historical materials, building consciously and conscientiously upon the work of others such as Pitirim Sorokin, Quincy Wright, David Singer, Melvin Small, and Frederick Woods and Alexander Baltzly. One hundred and nineteen wars involving great powers are identified in chapter 4. Chapter 5 details major variables: duration, extent, magnitude, severity, intensity, and concentration. It presents a clinical description of great power war as preparation for more analytical investigations presumably to follow. Chapter 6 is a summary of so-called "trends" in war. It presents scattergrams of some of the variables over time in order to provide some idea of the trends. Neither the graphics nor the description of them are particularly surprising. Using the date as a dependent variable, simple bivariate statistics are employed to examine a linear hypothesis of constant change in duration, extent, magnitude, severity, intensity, and concentration.

Although the inclusion of a considerable amount of quantitative material is commendable, most of it is what Edward Tufte would call "chart

junk." There are no really striking, non-obvious patterns. Scattergrams, bar charts, and bivariate correlations are unlikely to be satisfying to either the quantitatively or qualitatively oriented student of war. Cyclical analysis is suggested but dismissed as being "very complex" and not a justifiable expense in light of the visual absence of such trends in the bivariate displays. Chapter 6 ends with the examination of the major variables in five mutually exclusive, exhaustive time periods, yielding insights relating to which periods were the least war-prone (the eighteenth century).

The only hypothesis to be examined in this volume relates to the contagion of war, which is explored by calculating first-order autocorrelation coefficients (all of which are weak) and comparing the actual data to a Poisson distribution (the *chi-square* value suggests randomness, not contagion). Five-year aggregations and simple lagged correlations also indicate the absence of any regular, simple pattern.

Levy explicitly acknowledges both the contributions and the liabilities of the volume: It is a descriptive effort at collating some information on great power wars over the past five centuries. On the other hand, all of the information is presented at the systemic level and relates to the attributes of 119 great power wars. Thus, the effort is a macro-systemic one that ultimately must be oriented around a theoretical question. No such question emerges in this volume, and, as suggested in the Conclusion, it awaits further work. Several important and interesting findings are presented. For example, great power wars are more likely to emerge while another war is in existence, but great powers are not, apparently, especially mindful of the costs of the last war when deciding upon whether to enter the next one. What remains is for Levy and others to come up with plausible theoretical statements that can be empirically tested to explain these facts. It is that very difficult task of providing answers to the important "why" and "so what" questions that is missing in this important, descriptive volume.

Ultimately, we need to know if great power wars are a distinct social phenomenon, whether the underlying behavior is itself unique, what the characteristic dynamics are, and how all of this has been evolving in time. Levy's work will hopefully encourage additional inquiries of this sort.

MICHAEL D. WARD

University of Colorado

The Emergence of the NIEO Ideology. By Craig Murphy. (Boulder, Colo.: Westview Press, 1984. Pp. x + 190. \$22.00.)

To the states composing the Third World, nationalism and cultural autonomy has meant the right to develop their economies according to their own mix of political, social, and economic traditions and not to remain vassals to those countries who were first off the mark as far as industrialization was concerned. Why, as they would put it, must the American political experience and subsequent privileged economic position be enshrined as the perfect and transcendent model any more than the twisted neo-Marxist Soviet model? Central state planning, by the end of World War II, was a feature utilized by all of the belligerents, democracies and dictatorships alike. Further, why should not this tradition of using the power of the state to redress domestic, economic, and social inequalities be transformed into the international arena, so that the economic inequalities of the South in relation to the North have a chance of redress through the intervention of international institutions whose purpose would be not to ensure a free global marketplace preserving a historic division of labor based on traditional notions of supply and demand, or creditor and debtor, but to provide measures of discrimination (or non-reciprocity) in trade matters to favor the many rather than the few national actors?

The lesson that the Third World had already learned by the end of the War—and has relearned repeatedly ever since—is that the rich get richer and the poor get poorer if the rich are left to their own devices. Who, in other words, is to look after the poor besides the powerless poor themselves?

The New International Economic Order (NIEO) has been termed by some American political scientists who admire the Roosevelt New Deal and the Johnson Great Society as "global welfarism." If by this is meant linking economic and social pluralism (i.e., nationalism) with international institutions that would intervene in the "natural" international marketplace to redress the otherwise economic imbalance between rich and poor, then indeed the agenda of the NIEO was spelled out early, with the OPEC price rises of the 1970s merely adding political clout to a doctrine that was already well formed and already the object of dispute. As Murphy puts it in discussing whether the NIEO can be seen as a "new internationalism":

Critics who call the NIEO ideology 'mercantilism' can deal with the rights of states, at least as they are reflected in the western tradition of international law. But the NIEO ideology defines duties of states that go beyond respecting others' rights. And the

central concern of the new order ideology remains the activities of international institutions designed to help manage the world economy, something traditional mercantilists never thought of. (p. 165)

The emphasis on the positive duties of states—which is an argument that the weak always try to make against the strong—is well entrenched in the concepts of the NIEO and is resisted accordingly by those states that feel they might have something to lose. Why should the United States, as the Reagan administration put it, give away any theoretical or remotely advantageous self-benefit by creating new competition for itself in the Law of the Sea exploitation of deep seabed minerals through the creation of new international institutions that it might not be able to dominate? Why not keep the rules of the game in your favor as long as possible?

International institutions that work to favor the weak should themselves be kept weak or, even better, prevented from coming into existence. Alternatively, those that protect the strong should continue their useful protective function. Again, national bankruptcy must be avoided at all costs, even the endless rescheduling of interest payments (no mention of principal repayments yet) because of its threat to the lender. Even though debtor strikes, internationally speaking, have not yet taken place, they are not far from the surface in the negotiations concerning Latin American debt.

So far, although there have been some incremental adaptations of international economic institutions to third-world development needs, there has not yet been any fundamental restructuring of the international economy to provide the Third World with the essential NIEO ingredients of justice, equity, and autonomy. Dependency, in a world of interdependencies, still seems the order of the day. This book makes good, if sometimes difficult, reading.

ROBERT S. JORDAN

Naval War College; University of New Orleans

Law, Morality, and the Relations of States. By Terry Nardin. (Princeton, N.J.: Princeton University Press, 1983. Pp. xii + 350. \$35.00, cloth; \$14.50, paper.)

This is an important philosophical analysis of international law—and, to a lesser extent, international politics and organization—in terms of what Nardin identifies as the “practical” and “purposive” conceptions of international society. Obviously well-versed in the literature of his subject, Nardin calls upon a wealth of earlier analyses to buttress his own: “that morality and law are

best understood as authoritative practices constraining the pursuit of different purposes rather than as instruments for the joint pursuit of shared purposes” (p. ix). Nardin singles out Michael Oakeshott’s *On Human Conduct* (Oxford University Press, 1975) as the catalyst for his ensuing analysis.

In a rather difficult introductory chapter that demands a close reading, Nardin defines his terms and outlines his approach. Once his eclectic conceptualizations have been mastered, however, the rest of Nardin’s analysis, which is divided into three parts, falls into place rather easily.

If I understand Nardin’s theoretical position correctly, the main function of international law is practical, that is to regulate the relations of states possessing different and even incompatible goals, not purposive in the sense of achieving shared purposes. International society, argues Nardin, is “an association of independent and diverse political communities, each devoted to its own ends and its own conception of the good . . . which can only be enjoyed through participation in a common body of authoritative practices” (p. 19). This practical conception is “essentially constraint-oriented” (p. 14) and “is entitled to stand as the most adequate and illuminating of any of the conceptions of international society, morality, and law in terms of which the attempt to understand world affairs has been made” (p. 24). Although this final claim is certainly hyperbolic overkill, what follows is impressive.

Part 1 examines the gradual emergence of the modern state system “as a kind of practical association” (p. 49). It demonstrates how international law as a “body of rules specific to the relations of states” (p. 59) developed from an earlier understanding of the law of nations as a part of natural law and explores various experiments in international government.

Part 2 analyzes how international law can exist without such centralized rule-making institutions as a legislature, enforcement agency, or common judge. “It is possible to imagine law without legislation and law without sanctions, but impossible to imagine law without rules and obligations” (p. 133). Myres McDougal is taken to task for his purposive usage of international law to achieve “world public order.” “As an account of the character of international law,” McDougal’s conception “is unsatisfactory because it dispenses with the distinctions between rules and interests, making and applying law, and authority and efficacy upon which law, as a mode of association distinct from one governed wholly by considerations of interests and power, rests” (p. 206).

Part 3 examines international law in its moral aspect. In so doing it discusses some of the impli-

cations of the practical/purposive distinction for the concepts of international justice in the redistribution of social and economic goods, implementation of human rights, and waging of war. Here Nardin defends a practical conception of international justice against various purposive alternatives and objections. He writes, for example, that "to insist on respect for human rights is to demand that the policies and laws of a community reflect the principles of impartiality with respect to persons and their ends inherent in the idea of practical association" (p. 276).

In what I found to be maybe the most interesting discussion of all, Nardin discusses in his final chapter the relevancy of international morality and law for a culturally and ideologically diverse world. Although he acknowledges the existence of this diversity, which Adda Bozeman has so eloquently analyzed elsewhere, Nardin maintains that "the evidence accumulated through historical and anthropological study suggests the existence of certain striking moral similarities among otherwise dissimilar societies" (p. 320). He argues that "many of the conflicts between Western and non-Western states concerning international law can be explained by the different situations and interests of each, without resorting to the idea of a cultural gap" (p. 322). He concludes, therefore, that "not only are international law and morality possible in the face of ideological diversity, but they constitute practices peculiarly well adapted to helping states deal with the difficulties created by that diversity" (pp. 322-323).

Nardin has produced a stimulating analysis of international law within a heuristic framework that will amply reward the careful and thorough reader. His book is documented with useful footnotes throughout and contains a bibliography and index at the end. It should prove a major interpretation of, as well as addition to, the existing literature.

MICHAEL M. GUNTER

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The Making of America's Soviet Policy. Edited by Joseph S. Nye, Jr. (New Haven, Conn.: Yale University Press, 1984. Pp. x + 369. \$27.50.)

Why has it been so difficult for the United States and the Soviet Union to break out of the downward spiral of the cold war? Joseph Nye and his colleagues make two central arguments. First, although "American policy [is affected by] Soviet behavior . . . the fit is so imperfect that further explanation is necessary" (p. 6). The reactive component is not to be discounted totally, but

American foreign policy in general, and America's Soviet policy in particular, also are shaped in fundamental ways by their domestic roots—more specifically, their domestic political roots in the unique institutional structures, electoral practices, and ideological milieu of the American political system.

The book's second key argument is that these have not been especially nourishing roots. They have fed tendencies for "inconsistency and incoherence" in American policy (p. 334), for extreme pendulum swings between overemphasizing and overreacting to the Soviet threat, and at other times tendencies for self-deceptive underemphasis and unilateral restraint. The overswings to the Right have caused important opportunities for improving American-Soviet relations to be missed. The overswings to the Left have encouraged Soviet intransigence and provocations.

One of the most commendable features of this volume is how all of the chapters address these two themes. The lack of integration that weakens many other edited collections is avoided. These authors build upon each other's distinct expertises so that there is not only substantive comprehensiveness, but also an impressive overarching theoretical structure. The three chapters in Part 1, "The Actors," are exemplary. William Schneider elucidates the changes in American public opinion that have contributed to the oscillations of the past 20 years from cold war to detente to neo-cold war. He documents both the intransigent split between conservative and liberal internationalists, and the tension within the noninternationalist (but no longer inattentive) mass public, over the values of peace and strength. I.M. Destler finds the only consistency in Congress' role since 1972 to have been "to disrupt such purposive policies as the executive branch was pursuing" (p. 38). His analysis is sensitive both to issue area specific factors and to more general patterns of congressional behavior. Robert Bowie's wealth of policy world experience comes through in his critical discussion of how the executive branch has organized itself from Truman to Reagan for the formulation and conduct of Soviet policy. His advocacy of a strong Secretary of State system is more convincing than most.

Part 2 applies the inward focus to key policy subareas. Richard Betts draws the lesson that instead of repeating past counterproductive efforts to transcend domestic constraints on both nuclear arms control and competition, doves and hawks respectively should acknowledge that "treaties are not salable unless they appear to preserve military equality, and that unilateral efforts sufficient to substitute for treaty restraints on Soviet forces are not long sustainable" (p. 124). Alexander George's article on political crises adds a

historical context and a conceptual framework that inform the discussion with a sense of the inevitability but not immutability of American-Soviet international rivalry. Marshall Goldman and Raymond Vernon provide a well-reasoned indictment of anti-Soviet economic coercion, although the depoliticization they propose is hard to square with the evidence of domestic constraints provided elsewhere. Deserving of special mention is Strobe Talbott's powerful chapter on human rights, in which he shows the self-defeating consequences of overt American efforts to prod Soviet internal democratization. His identification of the tension between doing good and doing well (p. 200) gets to the very essence of the powerful causes and problematic consequences of American foreign policy's domestic roots.

In Part 3, Ernest May ("The Cold War"), Stanley Hoffmann ("Détente"), and Samuel Huntington ("Renewed Hostility") make the domestic causes and consequences themes the focuses for their historical reviews. These chapters whet the researcher's appetite for delving more deeply into the domestic politics of some of the specific incidents and policies they highlight. Dimitri Simes's chapter on the roots of the Soviet Union's "American policy" does not as neatly fit the volume's thematic structure, but does serve as a necessary complement.

Nye concludes with a synthesizing and prescriptive chapter. Nothing less than "a flexible strategy that combines a sensible vision of the future and a definition of our interests with appropriate means in a manner that can sustain domestic support" is called for (p. 341). This is quite the tall order. At certain points the reform proposals (both Nye's in this last chapter and the other authors' in their respective chapters) are left too vague; others are open to debate over their feasibility or viability. More important, though, is the success of the overall effort to develop what in another context Alexander George has called "policy-relevant" theory. In this and other ways this volume should be a model for others.

BRUCE W. JENTLESON

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The Arab-Israeli Conflict: Perspectives. Edited by Alvin Z. Rubinstein. (New York: Praeger Publishers, 1984. Pp. ix + 221. \$27.95.)

The pendulum of world attention rarely swings away from the Middle East for long. This is a realistic reaction because, in spite of being the subject of many clichés, the Middle East is in fact potentially the world's most continually explosive tinderbox. It is therefore not at all surprising that

scholars, journalists, and politicians continue to analyze and unravel the volatile Middle Eastern tangle.

The edited volume assembled by Alvin Z. Rubinstein attempts to further our understanding of what has been to date the most publicized aspect of the Middle East drama. The Arab-Israeli conflict is dissected in this volume as follows: In chapter 1, "Genesis," Adam M. Garfinkle sketches the pre-1948 background of the present hostilities. In chapter 2, "Seven Wars and One Peace Treaty," Itmar Rabinovich itemizes the bare details concerning the several major collisions between Israel and its Arab neighbors and inhabitants. In chapter 3, "Transformation: External Determinants," Rubinstein views the Arab-Israeli conflict within a broader set of circumstances ranging from the many technological advances in weaponry systems, through the U.S.-Soviet rivalry, to international terrorism. In "Israel: From Ideology to Reality," Alan Dowty examines the complexities and dilemmas of present-day Israeli political life. Aaron David Miller discusses what he refers to as "The Palestinian Dimension" in chapter 5. And in the concluding essay, Haim Shaked presents an overview designed to tie past, present, and future policies together.

The Dowty, Miller, and Shaked essays are insightful and provocative examinations of extremely complicated subjects. I found in them not only intelligent presentations of descriptive material, but also a keen sense for what the evidence may well mean.

Despite the noteworthy contributions of the three essays mentioned previously, in several instances I found the volume to be noticeably uneven in terms of the depth of treatment, and, overall, to be guilty of certain important sins of omission. There is a pronounced tendency to present a sketchy picture of events, to be more concerned with chronology and touching upon numerous points rather than thoroughly investigating any of them. In many ways the volume should be subtitled "An Introduction" rather than "Perspectives."

The lack of attention to political economy, either domestic or international, is bothersome. In addition, too little attention is directed toward how domestic and international events in the Middle East—specifically, the Arab-Israeli conflict—are parts of a puzzle in which the pieces taken individually acquire different texture and meaning than that which occurs when they are viewed holistically.

It is also not clear from this volume how we should read the Arab-Israeli conflict. Is it fundamentally a political matter with the frequent military overtones and exchanges being critical but

subsumed into larger nonmilitary, possibly even a non-Arab-Israeli, framework? Or, perhaps we should interpret the Arab-Israeli conflict as being a number of discrete, equally critical ingredients, with the military aspects being somehow separate and periodically ascendant? Several essays treat the conflict as a seemingly continual succession of military episodes while on the other hand, Shaked suggests that it is "a multi-issue, or multidimensional conflict" (p. 183). Although I do not want—and indeed would be most suspicious of—forced parsimony and simplified resolutions of complex matters, it would have been appropriate to have a more thorough, integrating treatment of these matters than was generally presented to the readers.

On balance, this volume contains more rewards than difficulties for the reader. In those instances where I found problems, there were important compensating qualities; and the essays by Dowty, Miller, and Shaked make valuable contributions.

CARL F. PINKELE

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International Dynamics of Technology. By Ralph Sanders. (Westport, Conn.: Greenwood Press, 1983. Pp. xiii + 332. \$35.00.)

The impact of technology and technological change on both international relations and national development has been pervasive. It is not so much that technology is out of control as the fact that the emergence of new technologies has created new global issues and made the management of political relationships more complex. Ralph Sanders attempts to capture how the interaction between politics and technology has created fundamentally new challenges for decision makers who are trying to maximize national interests and capabilities and minimize their vulnerability to technological changes outside of their immediate political direction.

Sanders suggests that there are three main reasons why the interactions between technology and international relations are important. First, an increasing number of the problems faced by humankind either are caused by technology or call for technical solutions. Second, the technological dimension of international relations is the least understood and appreciated by scholars and policymakers. And third, technological changes will continue to give rise to new and unique challenges that will demand additional knowledge and new skills on the part of national leaders.

For Sanders, the important point to get across is that the proliferation of technology-related issues is not a temporary phenomenon. Sanders

believes, correctly in my view, that these types of issues will increasingly occupy the attention of political leaders—who in many cases will lack the tools and capacity to deal with them effectively. Sanders's book represents an effort to alter that potential situation from occurring by highlighting the continued salience of technology in the economic, military, and political realms. In this regard, Part 4, which deals with institutions, strategies, and processes for managing technology, is particularly relevant.

Sanders's analysis points out that we may be witnessing a significant change in the conduct of diplomacy, which has occurred in response to technological imperatives. Nonetheless, he believes that "rather than being swept along by events, mankind can help shape the future by exploiting technological strategies among other devices" (p. 6). This may require, however, major improvements in the world's institutions and decision-making processes devoted to dealing with technological matters. In fact, Sanders suggests that "in the distant future the growing diffusion of technology worldwide could provide a potent counterforce to nationalism and thus contribute to restructuring the political organization of the planet" (p. 6). Although he does not push this argument too far, he might have acknowledged in somewhat greater depth that there are impediments to the widespread diffusion of technology within the presently constructed international system and that the constraints and opportunities associated with technology look much different from the perspective of the Third World than from the industrialized nations.

In the final analysis, Sanders acknowledges that "managing technology to gain maximum social, including international, benefit is an integral part of political wisdom" (p. 305). Unfortunately, at times he assumes that the political foresight and understanding will be there because of the imperatives created by the presence of technology. One could easily argue in the other direction, that is, we are seeing the emergence of a more neo-mercantilist world in which technologies are increasingly viewed as a national resource to be coveted, nurtured, and protected—but not shared. The rise in global competition particularly in the industrialized world, the growing concern about the increasing sophistication of exports from the newly industrialized nations, and the major differences within NATO and among many scholars and policymakers about whether technology can be used as a bargaining chip in East-West relations, leads one not to discount Sanders's argument about the salience of technology but to see it as a source of division and conflict.

Overall, one has to applaud Sanders for pro-

ducing an excellent piece of work, one that will be useful to scholars and practitioners involved in the field of foreign policy and international relations. Given the fact that new technologies such as computers and materials science have enhanced our problem-solving capabilities, it is easy to see how one might become optimistic about the readiness of political leaders to forge closer bilateral or transnational links in order to deal with global problems. Yet, the history of international relations over the last several decades indicates that the "wisdom" that Sanders hopes for may be severely lacking. Let us hope he is not as wide of the mark as he might be.

DENIS FRED SIMON

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The International Law of Pollution: Protecting the Global Environment in a World of Sovereign States. By Allen L. Springer. (Westport, Conn.: Quorum Books, 1983. Pp. xiv + 218. \$37.50.)

The purpose of Springer's book is to offer an analytical framework through which to study international environmental law and to suggest its prospects for the protection of environmental interests. He begins with a discussion of the range of environmental problems identified as appropriate for international concern and examines some of the scientific, economic, and political factors that make these problems particularly difficult to resolve on an international level. This is followed by a discussion of the role of international law as a framework for environmental protection and four major "schools of international environmental law, e.g., the traditional, transnational, modernist, and ecological imperative" (pp. 34-52).

Springer's basic argument is that the "international legal order has the capacity to resolve a number of significant environmental problems . . . through the implementation of treaty obligations and evolving norms of customary international law . . ." (p. xiv). As he admits (p. 54), his bias is toward the "traditional" approach as reflected in Hans Kelsen's *Principles of International Law* (Rinehart, 1952). Thus, it is the discovery and application of specific legal norms created by state practice and treaties that are the heart of his subsequent attention.

After first describing the general threshold at which environmentally damaging actions become legally significant, Springer discusses problems associated with making the threshold a viable legal concept. Specifically, he examines the ways in which concrete working limits for various kinds of detrimental change are established and given

legal expression before turning to current attempts by publicists to move away from traditional liability-based regimes of state responsibility in favor of approaches designed to create responsibility before pollution occurs. The final chapter is a case study of a proposal to build an oil refinery at Eastport, Maine, which illustrates the counter-thesis that nations encounter problems when they attempt to rely upon ad hoc negotiations (as opposed to external legal standards) to settle transboundary environmental disputes.

The book's major contribution is to advocate the relevance and the importance of the present legal system in preventing and/or settling environmental problems of international concern. The analytical "framework" that Springer utilizes is consistent with his adopted role of the international legal scholar as an "advocate" as opposed to a "social scientist" (p. 52). That is, he makes no attempt to analyze why scholars have questioned *whether* the present international legal system is sufficiently developed to resolve the disputes that arise over environmental issues. Rather, he assumes that rules of state responsibility, once in place, provide a major force for international environmental cooperation.

Unfortunately, the book's contributions beyond this advocacy position are rather meager. Springer disavows any attempt to analyze the "international legal order's capacity to respond to the threat posed by pollution," or "a comprehensive assessment of the steps that must be taken to enhance this capability" (p. 196). As such, his approach probably exaggerates the potential influence of international law upon global environmental management and adds very little to our ability to explain or predict international behavior. Other students, like Springer, take international environmental management as the policy outcome to be explained, and they offer generalizations that may be examined by themselves or by others. For example, they have suggested that levels of national economic development, domestic political structures (e.g., the state-society relationship), and/or international organizations are crucial influences on states' efforts to manage global environmental pollution. However, in this instance, international environmental scholars interested in further theory development will have to search elsewhere for testable generalizations. Some international legal scholars, on the other hand, may find the answers they are seeking in this book.

JAMES P. LESTER

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Alliance Security: NATO and the No-First-Use Question. Edited by John D. Steinbruner and Leon V. Sigal. (Washington, D.C.: Brookings Institution, 1983. Pp. viii + 222. \$28.95, cloth; \$9.95, paper.)

The Atlantic Alliance and Its Critics. Edited by Robert W. Tucker and Linda Wrigley. (New York: Praeger Publishers, 1983. Pp. vii + 192. \$24.95.)

Both of these anthologies address, in different ways, one of the central and long-standing dilemmas of the Atlantic Alliance: Over the decades, the United States has become compelled to deal with the Soviet Union on the basis of nuclear *parity* (as was reflected in the arrangements of the Strategic Arms Limitation Talks), whereas Washington can convincingly guarantee the security of Western Europe only on the basis of an implied American nuclear superiority. This is a dilemma for which neither the United States nor the Europeans can be blamed, nor is it likely that they can escape from it. The advent of nuclear parity between the superpowers has led to conflicts between the United States, which needs to take into account the potential nuclear devastation of the United States and therefore must qualify the use of nuclear weapons, and NATO partners at the forward line of defense who cannot accept a strategy that implies sustained conventional warfare at the expense of their territory and population. But aside from the security dimensions of that dilemma, both the United States and its European NATO partners (as well as the Soviet Union) perceive military-strategic developments as carrying a fundamental political import for the shape of the present and future European or political order. All parties express political purposes in the language of nuclear and conventional strategy and perceive the meaning of strategic doctrinal announcements primarily in political rather than purely military-strategic terms.

One of these doctrinal questions is that of the "no-first-use" of nuclear weapons. This is the focal point of *Alliance Security*, edited by Steinbruner and Sigal, in which a number of prominent American and European experts consider the political as well as military-strategic implications of a NATO doctrine that would deny the Western alliance the initiation of nuclear warfare. When McGeorge Bundy, George F. Kennan, Robert S. McNamara, and Gerard Smith published an article in the spring, 1982 issue of *Foreign Affairs* in which they concluded that no American president could responsibly initiate the use of nuclear weapons in defense of Western Europe, they challenged a doctrinal orthodoxy that had governed NATO's strategic thinking for decades. Because the no-first-use idea was, at bottom, directed

more toward the structural and political deficiencies of NATO than toward affecting the East-West military balance, it inevitably addressed long-standing issues that had concerned NATO over the decades and thus occasioned an intense debate among American and European public figures and strategic experts. NATO planners had always assumed Soviet superiority on the level of conventional forces and therefore the necessity for NATO to deter a Soviet attack with the threat to resort to nuclear weapons—the no-first-use idea, on the other hand, challenges both the premise and the conclusion of that assumption and calls for relying on conventional defense of NATO territory and a corresponding adjustment of NATO's deterrence doctrine. As a corollary, the no-first-use principle calls for a sharp distinction between conventional and nuclear military operations and a withdrawal of nuclear weapons from forward positions—reminiscent of the disengagement proposals of the late 1950s, one of which was presented by George F. Kennan in the fall of 1957 in the BBC lectures.

The arguments for and against a revision of NATO doctrine that are presented in *Alliance Security* are marshalled with sophistication, clarity, technical expertise, and circumspect political judgment. David N. Schwartz provides a historical perspective of NATO's doctrines and the corresponding force posture adjustments; and William W. Kaufmann deals, in two chapters, with the conditions and limits of strategic and tactical nuclear deterrence and with the possibilities for a nonnuclear deterrence posture that emerge from the NATO-Warsaw Pact military balance. Jonathan Alford (in a few pages) and Leon V. Sigal (in two chapters) examine the relationship between stable deterrence and the no-first-use principle, with Sigal arriving at the conclusion that the no-first-use strategy, inasmuch as it calls into question America's willingness to use nuclear weapons in Europe's behalf, would give "undue advantage to those who would oppose any change whatever" and thus "only delay the implementation of essential changes in NATO's posture and plans" (p. 145). Gert Krell, Thomas Risse-Kappen, and Hans-Joachim Schmidt examine West German attitudes on the no-first-use question, a subject of central importance because in many respects NATO has been reduced to a bilateral U.S.-FRG security connection, and because the German position on the issue is governed by the particularities of the Federal Republic's geography, history, and its special relationship with the German Democratic Republic. In the concluding chapters, Johan Jorgen Holst assesses the practical obstacles standing in the way of implementing the no-first-use principle, and John D. Steinbruner offers

some general reflections on Alliance security and on matters of disagreement that are sharpened by the no-first-use debate.

The essays that comprise the volume edited by Tucker and Wrigley, *The Atlantic Alliance and Its Critics*, were presented initially at a seminar series sponsored by the Lehrman Institute during 1981 and 1982. Their focal point is the political rather than the military-strategic dimension of transatlantic issues. Theodore Draper and A.W. DePorte provide a long-term perspective on the Atlantic Alliance, with Draper arriving at rather pessimistic conclusions about the past performance and future prospects of the Alliance, whereas DePorte believes that it has over the decades served some of the most important interests of its members reasonably well. Pierre Hassner sharply disagrees with DePorte's central thesis that the postwar division of Europe has solved the German problem, and he examines also

the domestic social and political conditions that affect Alliance cohesion. Simon Serfaty and Pierre Lellouche deal with the future of the Alliance, both seeing the need for structural changes and a fundamental political redirection that would reflect a more sustained and self-conscious consideration of European regional political interests—a view that is not shared by Robert W. Tucker, who suggests in his concluding remarks that the corrosive effects on the Alliance will continue so long as Europe remains as committed to detente as it is to the Alliance.

Both volumes are required reading for anyone interested in the future of the Atlantic Alliance in a decade in which the foundations laid for the transatlantic compact in the postwar era are visibly eroding.

WOLFRAM F. HANRIEDER

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Normative Theory

After Marx. Edited by Terence Ball and James Farr. (New York: Cambridge University Press, 1984. Pp. x + 287. \$37.50, cloth; \$11.95, paper.)

Commemorating the centenary of Marx's death, this collection of 12 essays on Marxian theory is based on the view that Marxism is a uniquely fruitful, but often inadequate, ambiguous, and unfinished body of social and political theory. The essays tend to be sympathetically critical of Marx and Marxism, and most offer reformulations or new accounts of troublesome areas. A unique offering of this collection is that most of the authors are conversant with non-Marxian schools of social theory and philosophy, often allowing them to experiment with eclectic and sometimes innovative approaches.

The essays are divided into three thematic sections. The first, "History and Revolution," deals with issues surrounding Marx's explanations of history. A strong essay by William Shaw, "Marxism, Revolution, and Rationality," argues that recent attempts to show the untenability of Marx's views of revolution in terms of prisoner's dilemma game theory rely on a narrow instrumentalist conception of rationality as well as on unsupportable psychological assumptions. An essay by Jon Elster, "Historical Materialism and Economic Backwardness," proposes a general theory of

economic backwardness to show why—contrary to Marx's expectations—socialist revolutions have occurred in backward countries while advanced countries have developed resilient forms of capitalism. The result is a rather disappointing two-dimensional typology combining economic and motivational variables rather than anything like an explanatory theory. Two well-argued articles in this section deal with Gerald Cohen's recent and influential attempt to construe Marx's historical explanations in functional terms. Philippe Van Parijs's "Marxism's Central Puzzle" assesses Cohen's approach in light of an evolutionary theory that would show how reciprocal causality (the "central puzzle") is possible in Marx's base-superstructure model. And in "Marxian Functionalism," James Nobel suggests that Marx's conception of history not only relies on functional explanations (as Cohen argues), but also provides the concrete accounts of change that justify such explanations (which Cohen believes Marx lacks). Finally, Richard Miller's "Producing Change: Work, Technology, and Power in Marx's Theory of History" argues that Marx's explanations do not invoke technological determinism, but instead rely on the broad political and economic causalities expressed in the concept of mode of production. Miller's essay is perhaps the weakest of the collection: His version of technological determinism is a straw man, and

he seems unaware of structuralist interpretations that parallel his own with more force and clarity.

The essays in the "Morals and Politics" section develop the contours of Marx's political theory. A solid essay by Paul Thomas, "Alien Politics: A Marxian Perspective on Citizenship and Democracy," locates Marx within the tradition of Aristotle, Rousseau, and Hegel that emphasizes—*contra* liberalism—the positive aspects of politics. C.B. Macpherson's competent and lucid "Democracy: Utopian and Scientific" repeats his earlier criticisms of pluralism (it is neither very scientific nor democratic) to suggest a return to pre-liberal democratic ideals in the context of a science like Marx's. In "Marx's Moral Realism: Eudaimonism and Moral Progress," Alan Gilbert argues that Marx is a moral realist comparable to Aristotle. Although Gilbert is surely right that Marx possessed a systematic ethics embodying Aristotelian features, the "realist" label (borrowed from contemporary analytic philosophy) suggests a metaphysics quite foreign to the critical and reflexive elements of Marx's philosophy. Finally, John Roemer's "Exploitation, Class, and Property Relations" makes a remarkable contribution toward specifying economic exploitation in feudal, capitalist, and socialist societies in terms of a general model that focuses on property relations.

The "Methodology and Criticism" section is tightly organized around distinguishing Marx's reflexive materialism from the positivistic materialism of Engels. James Farr's well-argued "Marx and Positivism" emphasizes the incompatibility of Marx's philosophy with positivism. Terence Ball's "Marxian Science and Positivist Politics" underscores the connection between Soviet social engineering and Engels's positivistic interpretation of Marx and includes an excellent section on the Hegelian roots of Marx's philosophy of science. In "Marxism and Method" Terrell Carver seeks to dispel the association between the "dialectical method" that Engels borrowed from a cursory reading of Hegel's *Logic* and Marx's own "methodological pluralism."

If the failings of several of these essays can be traced of any one source, it is in the apparent non-conversance of their authors with parallel problems in critical and structuralist Marxism. Their collective success lies in a vigorous broadening of the terms of discourse surrounding Marx and Marxism, and this is no small achievement.

MARK WARREN

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Happiness, Justice and Freedom: The Moral and Political Philosophy of John Stuart Mill. By Fred R. Berger. (Berkeley: University of California Press, 1984. Pp. x + 364. \$24.95.)

This book represents an attempt to rescue the thought of John Stuart Mill from the serious criticism that has been levelled against it for more than a century. As Berger explains, much of the confusion surrounding Mill stems from the manner in which he rejected the Utilitarianism of Jeremy Bentham. Bentham claimed that the sole scientific means for differentiating between activities is the quantity of pleasure they produce. He also maintained that he was not interested in the intrinsic worth of activities, only their final results. What Mill recognized is that to scientifically compare competing courses of action, they must be of the same order. Evaluating qualitatively different acts, on the other hand, involves bringing standards other than pleasure into the equation and taking intrinsic values into account.

Having disposed of the specific creed of Utilitarianism so effectively, Mill proceeded to insist that utilitarianism in general need not fall prey to such illogic. In 1863 he wrote: "It is quite compatible with the principle of utility to recognize the fact, that some *kinds* of pleasure are more desirable and more valuable than others." Critics have been pondering this assertion ever since. How can a theory which by definition denies the validity of intrinsic worth and considers phenomena by their pleasurable effects alone, they have inquired, distinguish consistently between different orders of pleasure?

Berger believes that Mill offered a reasonable utilitarian alternative to Benthamite hedonism and classical metaphysics. He develops that alternative with reference to the Millian view of human nature. According to Berger, Mill held that the most virtuous choices afford men the most pleasure. By his account, not only did Mill attribute pleasure to the consequences of virtuous acts but also to their contemplation before action. In this fashion, Berger concludes, Mill was able to disengage himself consistently from Bentham, deny that phenomena possess innate worth, and still take qualitative differences seriously.

Berger also brings his considerable analytical skills to bear on Mill's ambiguous "General Happiness Principle," ambivalent theory of justice, and famous defense of individual freedom. These, he argues, are compatible with the Millian conception of man as well. Berger's tone is balanced and nonpolemical throughout. He stands by Mill when he feels his thought is not treated fairly, yet is willing to give the critics their due when they appear to be correct. Thus, although Berger is confident that he has identified a significant link be-

tween Mill's view of human nature and his other controversial ideas, he does not refrain from reproving that view for being overly optimistic.

ETHAN M. FISHMAN

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Wittgenstein: A Social Theory of Knowledge. By David Bloor. (New York: Columbia University Press, 1983. Pp. xi + 213. \$25.00, cloth; \$12.00, paper.)

Ernest Gellner once remarked that anthropologists invariably become Tories for the cultures they study. A similar insight motivates this thoughtful book on the foundations of Wittgenstein's mature philosophy and its relation to the human sciences. Bloor's central contention is that Wittgenstein's remarks on language games and the foundations of knowledge presuppose a naturalist account of social needs in terms of which the nature and evolution of language games can be explained, and his main constructive task in this book is to provide the outlines of such an account. Drawing on the work of anthropologist Mary Douglas he develops a comparative typology of language games based on how they respond to anomalies—whether by excluding anomalous information or practices, or adjusting to them; and if the latter, by what internal mechanisms. Some such account is required, in Bloor's view, because Wittgenstein's notion of forms of life is at bottom sociological, yet Wittgenstein and his anti-naturalist followers have shied away from grappling with sociological—and indeed all causal—questions about language. In order to demonstrate this claim Bloor undertakes a wide-ranging discussion of Wittgenstein's views on the mind/body problem, private languages, universals and family resemblances, the foundations of logic and mathematics, and the philosophy of action. He argues first that Wittgenstein's few remarks on creativity do not begin to explain why language games adapt and evolve in the ways that they do, and, second, that those (such as A.I. Melden) who try to avoid this issue by arguing that causal explanation is irrelevant to the philosopher's task of elucidating *meaning* beg the question. Indeed, for Bloor, the whole debate between those who advocate causal explanations of linguistic actions and those who do not is misleading as well as contrary to the antireductionist spirit of the Wittgensteinian enterprise. Bloor contends that we play many different types of language games (often simultaneously), and that these are governed by different rules and are susceptible to different types of explanation.

Bloor is aware that there is a tension, at least,

between this claim and his own attempt to come up with a comprehensive comparative sociological theory of language games. Wittgenstein eschewed such ontological system-building precisely because he practiced what J.L. Austin preached: He collected linguistic distinctions like one collects beetles, and he thought anything else ultimately misguided. But Bloor is not content with this. Turning the antireductionist case against Wittgenstein, he argues that the kinds of causal explanations of linguistic actions that interest social scientists are themselves parts of our language games and forms of life which we can only ignore (at our peril) by illicitly reducing everything to the cognitive plane. Despite their distaste for all things empirical, ordinary language philosophers are not being true to their own *descriptive* enterprises when they fail to take account of the fact that language games function on a variety of interrelated planes which are "superpositioned" on one another, and that the relations between, for example, cognitive and social planes may be critical to understanding a language game which ranges over both. For example, on the cognitive plane, the statement "I do" in a marriage ceremony might be nothing more than an authentic promise; on the sociological plane, it might be an act of reproducing the nuclear family. If so, an elucidation of the meaning of the action will be incomplete and misleading if it ignores this latter. It is for this sort of reason that Bloor goes on to argue, as opposed to Hanna Pitkin and others, that there is an inherent political conservatism to the Wittgensteinian project: Its point of reference is always the prevailing conventions in a culture. By appealing to these as the ultimate means for understanding and evaluating what we do, Wittgenstein is necessarily making a conservative appeal. These are powerful, if not entirely novel, arguments, and it is worth noting that they stand independently of the particular comparative theory Bloor wants to advance.

With the exception of the penultimate chapter (on Wittgenstein's political conservatism) which is schematic and difficult to follow, the book is lucidly and accessibly written. It could profitably be used in advanced undergraduate and graduate courses on the philosophy of social explanation and the study of ideology.

IAN SHAPIRO

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Coherent Variety: The Idea of Diversity in British and American Conservative Thought. By Michael D. Clark. (Westport, Conn.: Greenwood Press, 1983. Pp. viii + 228. \$35.00.)

With the political resurgence of the Right in this decade, conservative political thought is certainly a timely subject. The conservative tradition, (like the liberal or radical ones) perhaps as a product of engagement with political controversy, is laced with seemingly inconsistent themes or ideas. This has led many persons, even some of its friends, to conclude with Clinton L. Rossiter that "Conservatism is self-contradictory. How can the Conservative preach both the inviolability of the person and the primacy of society, the ascendancy of moral values and concept of property as a natural right, the aristocratic spirit and the brotherhood of men as children of God?" (*Conservatism in America*, 2nd ed. rev., Vintage Books, 1962, p. 60). On closer examination, however, many such apparent inconsistencies reflect reliance on some underlying notion like hierarchy or community. Michael Clark examines a theme not often associated with the conservative tradition, that of diversity, and illustrates the way it ties together somewhat inconsistent ideas and thinkers within this tradition.

The concept of diversity seems more closely linked with such liberal celebrations of individual autonomy as Mill's *On Liberty* than with conservatism. Yet Clark carefully demonstrates the way diversity is presupposed by several core values of the tradition. In the first part of the work, for example, he details the implicit appeal to diversity involved in the historic grounding of conservatism in respect for religion and aversion to intellectual and social disorder. The prominence of ideas like "The Great Chain of Being" and "social harmony" in the works of early European conservatives, he argues, bespeaks a subtle affirmation of the notion of diversity captured in the notion of "unity in variety." It is respect for diversity, Clark further suggests, and a resulting distaste for any emphasis on uniformity and reductionist tendencies that largely account for conservative hostility to rationalism, rather than anti-intellectualism per se as some have charged.

In the second part of the book, Clark traces the notion of diversity first held by British, then American nineteenth-century conservative writers. He concludes this part of the book with a discussion of the role of diversity in the thought of Southern conservatives, where he confronts the way in which the concept has been used in patently reactionary formulations. In the third and final part of the book, Clark brings the discussion up to the present and contrasts conservatives with other critics of uniformity and the conformist

tendencies of the modern era.

As an intellectual historian, Clark makes his argument through an exhaustive survey of political writers over the last 200 years. This is at once a weakness and a strength of the book. As a result of this approach, his treatment of each author is highly compressed, at times verging on the superficial. His provocative placement of William James in conservative ranks (pp. 152-157) fairly begs for a more careful analysis, for example. Yet his purpose is to establish diversity as a basic conservative concept, and by means of this survey, he achieves this end. Using the idea of diversity, Clark not only relates a wide variety of thinkers in a plausible way, but also develops some interesting conceptual insights. One characteristic of a rich and significant work is the extent to which it suggests many avenues for further exploration. If only on this basis, political theorists and others interested in understanding one of the major intellectual and political movements of our time will find this work valuable.

JOHN D. HARMAN

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Political Leadership and Nihilism: A Study of Weber and Nietzsche. By Robert Eden. (Gainesville: University Presses of Florida, 1984. Pp. xx + 348. \$25.00.)

This intriguing critique of attempts at "building a systematic politics upon the foundation of a concept of leadership" (p. xix) contains several original contributions to philosophical and political discourse. First, Eden provides an intelligent examination of Nietzsche's philosophical politics and a cogent argument for reckoning with this "vulgate" Nietzsche. Second, his intricate dialogue between Nietzsche and Weber regarding the place of political leadership in a nihilistic world is the most comprehensive consideration of their intellectual interaction to date. Third, Eden uses this comparison to suggest that in its reliance on leadership American reform liberalism contains not only the seeds of its own destruction (now sprouting in neoconservatism), but also those of liberalism per se.

Eden's argument is tightly, even densely, woven. He begins analyzing Woodrow Wilson's successful substitution of "reform" for "leaderless" liberalism and emphasizing the absence of theoretical limits upon individuality in a Wilsonian world. In this context, Eden introduces Weber's attempt to impose practical limits upon individuality and thereby to preserve liberal politics once liberal principles have become obsolete (chap. 1). Although Weber's emphasis

upon self-imposed moral values seems attractive (especially, Eden fears, to American liberals recently aware of Wilson's dangers), Eden argues that ultimately it too is flawed. He develops this argument through a dialogue between Weber and Nietzsche in which the former tries to defend morality in the latter's nihilistic world. The dialogue begins in chapter 2 with Nietzsche's account of individuality as the core of liberalism and science. What follows is an examination of conflicts between liberalism and science that flow from this common origin: Nietzsche's scientific conquest of morality overcomes even liberal morality (chap. 3) and leads to an aristocratic politics subordinate to new philosophers (chap. 4). Weber responds to Nietzsche's attack upon liberalism by trying to turn science (chap. 5) and politics (chap. 6) toward "moral forces," but he can only marshal psychological resistance. He cannot call in philosophical reinforcements because he concedes to Nietzsche that political philosophy is no longer possible. Eden only briefly discusses Weber's additional reinforcements, that is, "empirical" or "given" limits to leadership. Yet he maintains that Weber was ambivalent about their efficacy and based his argument for moral restraint not upon "given," but upon "constructed," facts (pp. 218-219).

Eden concludes that Weber's attempt to arrest Nietzsche's attack upon liberalism in *medias res* fails because the moral leadership on which Weber depends destroys the moral limits he would defend. To his initial question, "What becomes of liberal politics if we abandon the plane of theoretical understanding as Weber and Nietzsche do, to reconstitute the liberal cause on the plane of practical historicism?" (p. 35) Eden responds, "One must either rely directly upon political philosophy, and defend liberal democratic institutions by recovering the legislative science of modern liberal politics, or give way before Nietzsche's alternative" (p. 219).

Eden clearly chooses the former path, but his philosophic defense of liberal democracy misses the thrust of Nietzsche's challenge and the power of Weber's response. Eden calls for "constitutionalist suspicion" to be directed toward a critique of political leadership (p. 237). Yet he does not explain why Nietzsche's science spares his (our) constitution, or how that constitution differs from Weber's "empirical" and "constructed" facts. (Not to mention whether it escapes Wilson's critique of private government—the impetus for reform liberalism—with which Eden began.) In short, Eden (like Weber) fails to establish philosophical criteria by which the committed liberal can defend his truth against opinion. Perhaps he (again, like Weber) finds empirical "givens" bolstered by "constructed" facts

the best possible defense for morality. This in itself is not cause for criticism. However, Eden's claim to ascend from opinion to truth, and not to hover somewhere in between, mars an otherwise fascinating work. One is left with the impression that he blames the messenger and refuses to hear the news.

NANCY S. LOVE

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The Political Theory of *The Federalist*. By David F. Epstein. (Chicago: University of Chicago Press, 1984. Pp. ix + 234. \$22.00.)

The Authority of Publius: A Reading of *The Federalist Papers*. By Albert Furtwangler. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 151. \$14.95.)

Jefferson called *The Federalist* "the best commentary on the principles of government, which ever was written," and Americans ever since have remained convinced of its merits. But the work has perhaps suffered from an excess of veneration and of familiarity. Perhaps a fresh start needs to be made, and the essentials of *The Federalist* recovered. Both of these books have made the attempt. The authors have, however, approached Publius from very different vantages, and thus have produced quite different results.

Furtwangler's book contains some promising insights, but its argument fails to develop or think them through. It disappoints because in attempting to be comprehensive, it leaves much half-argued or asserted without evidence. Furtwangler's basic premise is that any interpretation of *The Federalist* must take into account its origins in the popular press, where it was one among many pseudonymous series. Whence, then, came its unique authority? He concludes that the real achievement of *The Federalist* was to set a tone of "literate civility" for future deliberations on constitutional and public questions down to the time of Lincoln and Douglas (p. 147).

This is a thesis worthy of investigation. It calls for a history of Publius' antecedents in the press, a comparison of Publius with his opponents and his rivals, and a demonstration that Publius was taken as a model by others in discussing political questions before the American public. Such a book might well bear out Furtwangler's contentions.

But this is not the book he has written. Instead he spends much energy early in showing that conventional views of the authority of Publius are untenable. He does not establish an interpretation of Publius' teaching, and thus the rest of the book reflects an uncertainty about it that limits the case

for its authority. For example, Furtwangler argues that in *The Federalist* Madison and Hamilton attempted as much to explain the Constitution to themselves as to the people. It "reveals not only the voters but the framers in the act of deliberating. This, I believe, is the most enduring source of its authority" (p. 87). But given Furtwangler's ideas about the limits of reason (and therefore deliberation) in politics, we must wonder how authoritative that is (compare pp. 111, 148). The final chapter on *The Federalist* No. 10 is gratuitous and breaks no new ground, with the exception of a highly problematic section showing similarities between the essay and psychologist Erik Erikson's findings about the American family.

Central to Furtwangler's analysis is his claim that *The Federalist* lacks a systematic understanding of politics, that is, a "theory" (p. 43). This claim is denied in the very title of Epstein's book, and Epstein gives a most detailed and scholarly proof that Publius had a political philosophy worthy of our most serious attention.

Epstein challenges those who assume that *The Federalist* is merely a reflection of modern political philosophy: *post Locke, ergo propter Locke*. But he also argues against those who find in Publius the legacy of a "classical republican" tradition, pointing to the book's novelty as the first to recommend a wholly popular government. To sustain these challenges Epstein subjects the argument of *The Federalist* to an exhaustive analysis. Publius, he finds, does not rely solely on mechanical devices or on republican public-spiritedness to solve the problems of popular government. Both interest and virtue are deficient, when taken alone.

Epstein understands *The Federalist* to be divided into two main sections—sections that coincide with the division of the first collected edition into two volumes. This division is quite natural, and Epstein makes good use of it in his analysis. The first volume (1-36) considers "the fundamental issues of liberalism," that is a government that will meet the necessities posed by "accident and force" (p. 8). Volume 2 (37-85) explains the requirements of good government and how the new Constitution meets those requirements.

The treatment of the first volume culminates in a detailed (51-page) analysis of *The Federalist* No. 10. Epstein shows that the improvements Madison proposes in the modern science of politics protect man's capacity of forming political opinions as well as his acquisitiveness. It is commonly supposed that Madison's improvements in the science of politics were a refinement of the mechanism of government depending entirely on the acquisitive faculty for its motive force. But Epstein notes that scholars have paid insufficient attention to the first of the two

kinds of faction named by Madison, that moved by an "impulse of passion." Passion is capable of overturning even the most finely tuned mechanical system. It is Madison's improvement on modern political science that man's "faculty of passionately opining" retains a place in the regime. The operation of this faculty is expanded upon in the second volume, where ambition and honor figure more prominently. The particular structure of the government and its branches allows man's political nature to manifest itself without compromising good government (p. 125). Epstein's demonstration that Publius did not simply articulate interest-group pluralism, but a regime in which prudential judgments would always have to be made, is the book's most important contribution.

The detailed analysis of the text of *The Federalist* is likewise a valuable contribution. Epstein does not make the mistake of trying to evaluate all of the previous major commentaries on *The Federalist*. Yet he does take their arguments into account, and his research is quite thorough. But a more systematic presentation of Epstein's fundamental thesis would be very helpful. Also, some connections that might have been made, for example, between justice, the public good, and the "fundamental principles of the revolution," are not fully made (p. 163). Missing, too, is a discussion of founding—which forms an important, if not critical, theoretical section of the book—and particularly founding as an activity calling forth the highest kind of statesmanship. A treatment of essay 40, where Publius discusses the work of the Convention, might have added much power to Epstein's analysis. But not everything about Publius can be said by one author in one book; it is an accomplishment to have started us thinking about Publius. Epstein's fine contribution is indispensable reading for anyone seeking to understand *The Federalist*.

J. JACKSON BARLOW

The New Federalist Papers

A Commentary on Plato's Protagoras. By Larry Goldberg. (New York: Peter Lang, 1983. Pp. 352. \$33.70, paper.)

Goldberg proposes a study of Plato's *Protagoras* that pays due attention to the dialogue's dramatic context. His procedure appears thematic, with separate chapters on the dialogue's characters—Protagoras and Socrates, but also Simonides—with a chapter on Measure and Community and with a conclusion intended to identify the dialogue's structure. But after the first chapter, this approach falls generally into

abeyance as Goldberg pegs his observations to the course of the dialogue.

Two telling features of Protagoras are his secretiveness and his use of flattery, both of which are necessary because Protagorean sophistry is no true friend of democracy. Despite an egalitarian argument affirming the political capacity of all citizens, what Protagoras in fact promises is rule by the wise. Also noted is the fact that Protagoras is circumstantial and discrete; he is quick to isolate and divide, and thus dispositionally not inclined to see virtue as one.

Socrates sets about to expose Protagoras, not merely the deficiency of his wisdom, but the essential contradictoriness of his character, especially his disdain for—and his likeness to—the many. Socrates' subsequent investigation of the unity of virtue, which seems to be a new beginning, is instead an oblique attack on Protagoras' long oration. The effort by Socrates to establish the oneness of virtue comes close to exposing the injustice of sophistry and thus forces Protagoras to employ rhetorical devices to save himself. The problem facing Socrates both here and elsewhere is that in attacking Protagoras, he looks to be a sophist himself.

In the Simonidean section of the dialogue, which Goldberg gives greater attention to than previous scholars, Socrates is seen to improve upon Simonides' ode with a deliberately errant interpretation, to raise the problem of agreement in discourse, and finally to return the discussion on a higher plane to the question of virtue's teachability. One of Goldberg's concerns here is to determine Simonides' relationship to Protagoras (Simonides is called a sophist by Protagoras) and to Socrates (Socrates undertakes to defend Simonides).

In the closing section of the dialogue, Goldberg examines all of Socrates' arguments for why courage cannot be divorced from knowledge. He maintains that the last of these arguments which introduces hedonism is the most complete, but that the hedonistic assumption is not to be associated with Socrates. It more fittingly belongs to Protagoras, who is further encouraged to make knowledge a more prominent part of his sophistry. The ultimate objective is some sort of political community comprising both the wise few and the demos.

By way of criticism, I would say that Goldberg's commentary suffers from some, although not many, reflections that are at least unnecessary if not unwarranted. His observations on the physician—good, bad, and middle—strike me this way. More seriously though, his commentary lacks a unifying thesis. Various themes, such as community, improvement on the sophist, mode of discourse, and nobility, do surface, but they are

not pursued with the regularity needed to disclose the dialogue's structure, and the concluding chapter that is assigned this task is mainly concerned to show parallels (albeit instructive ones) between the *Protagoras* and the *Clouds* of Aristophanes. Hence, Goldberg cannot spare the reader from asking of the end, "Exactly what is the *Protagoras* all about?" This is a question, however, that might legitimately be put to many of the dialogues, and so the criticism is not meant to be overly harsh. Goldberg's commentary is in fact a worthy piece of analysis and a useful contribution, especially because Platonic scholarship today is dominated by a narrow regard for logical consistency.

PATRICK COBY

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Mill on Liberty: A Defense. By John Gray. (Boston: Routledge & Kegan Paul, 1983. Pp. xiii + 143. \$18.95.)

John Stuart Mill and the Pursuit of Virtue. By Bernard Semmel. (New Haven, Conn.: Yale University Press, 1984. Pp. xi + 212. \$17.50.)

Few of the upheavals in political theory in recent years seem to have affected the reputation of J.S. Mill. Although the general focus in the field has shifted backwards somewhat (especially under the impact of Pocock and Skinner), Mill, with Marx, has remained symptomatic for many of the central positions of both nineteenth- and twentieth-century political debate. Both of these books remind us, however, that there are by now nearly as many Mills as there are Marxes, and that the process of interpretive proliferation is by no means yet ended. For Bernard Semmel, Mill was a more conservative political thinker and actor than is generally assumed, as well as more of a classical democrat in his emphasis on virtue than an enthusiast for the political nonparticipation fostered by commercial society. Early on, Semmel's Mill adopted an essentially activist, Stoic-Christian notion of virtue rather than seeking to transform a more passive Epicurean (and Benthamite) concept of pleasure into an ideal of virtue. Wishing to preserve a sphere for individual effort amidst the then-intense debate over "free will" versus "necessity," Mill in this account (and its emphasis is a very useful one) attacked the fatalism of the Owenites in his youth, and continued this line of criticism later against the Saint-Simonians, Comte, Buckle, Hamilton, and others. Without succumbing to something like Carlyle's concept of the "great man," Mill was concerned to furnish a philosophical ground and offer political protec-

tion for an elite able to ensure moral and social progress through their own excellence.

This concern for virtue and the virtuous led Mill not only to his well-known endorsement of the Hare system of proportional representation to ensure political power to an educated elite, but also to positions dissenting from the Radical party in parliament on such matters as capital punishment (Mill remained in favor) and the Crimean War (which he supported). Semmel does not directly connect Mill's adoption of such views to his "pursuit of virtue," but it is apparently the basis of the relative conservatism imputed to Mill in the Introduction and elsewhere in the text. In taking up the conception of self-development from German idealism, rather, Mill's politics are in Semmel's eyes far more those of the modern social democrat than of the conservative or noninterfering liberal.

For Semmel, this is nowhere more clear than in *On Liberty*, which is interpreted not as a paen to "negative liberty" (against Stephen, Arnold, and many others), but as a plea for the "positive liberty" of self-development, public participation, and self-realization (p. 166), an ideal that does not contradict the demands of Mill's principle of utility even if it does enjoin a preference for higher over lower pleasures. The possibility of a potential conflict between the principles of liberty and utility is also the starting point for John Gray's closely argued attempt to exonerate *On Liberty* from many of the deficiencies usually attributed to it. Arguing in particular against James Stephen, Gray allows that such a conflict cannot arise if we presume Mill to have intended his principle of utility to be essentially a principle of evaluation rather than one directly commanding right action. Claiming that men could never be happy so long as they directly pursued happiness as an object, Mill was hence an "indirect" and not an "act" or "rule" utilitarian, whose principle of utility is therefore not a moral principle of the same kind as liberty, and thus cannot conflict with it in most traditionally assumed ways.

Like Semmel, Gray sees a Stoic idea of freedom as self-determination as lying at the basis of Mill's notion of "autonomy" (the term is Gray's), the idea of individuality defended at length in *On Liberty*. The doctrine of liberty for Gray does not imply any particular theory about the proper limits of state activity, or a special commitment of *laissez-faire*, and as such is compatible with some variants of socialism and social democracy. What is important is that there are no "coercive" limits placed on liberty, not that the principle of negative liberty be expressed in absolute terms. Following Rees, Gray presumes that when Mill limits liberty to not harming others, he means harming their interests, the two most essential of which are security and autonomy. This is the basis

for Mill's theory of moral rights, with the domain of freedom of action being equal to the sum of these rights, and the core of Mill's doctrine of liberty is thus that utilitarian grounds can be given for defending the vital interests of autonomy and security in this manner.

Both of these books therefore defend Mill's value and achievements against his critics. As a more general account, Semmel is particularly strong in his focus on *how* Mill sought virtue, but offers no close analysis of any particular text or new material on Mill's biography. Gray can be read best in conjunction with C.L. Ten's *Mill on Liberty* (Oxford University Press, 1980), which tells us more about what Mill's critics have found in the text, but is itself a less tightly woven argument. Both works clearly demonstrate that a social democratic Mill may yet have much to offer the late twentieth century, and that Mill's reputation remains assured.

GREGORY CLAEYS

University of Hanover (West Germany)

Hannah Arendt: Politics, Conscience, and Evil.

By George Kateb. (Totowa, N.J.: Rowman & Allanheld, 1983. Pp. 204. \$24.95.)

George Kateb has written a provocative, very personal, and at times intellectually agitated study of Hannah Arendt's political thought. Kateb acknowledges Arendt's unique capacity to stimulate thought on a range of discomfiting questions from the nature of politics to the evil of totalitarianism set against the crisis of modernity. He captures the spirit of Arendt's politics by demonstrating that her "life's work is in defense of freedom" (p. 196). He recognizes, although doesn't affirm, Arendt's enormous ambition to recover the lost "treasure" of political action (p. 7). Kateb's explication of Arendt's understanding of political action stands among the best we possess. That power of explication, however, reveals the core passion of the book and the foundations of Kateb's agitation. He defends political philosophy as moral speculation, and representative democracy as a unique form of moral life, against Arendt's subversion of them. Accordingly, Kateb finds the practical import of Arendt's political thinking painful, shocking, blasphemous, and "foreign whenever it is seriously attended to, certainly in America" (p. 1).

Kateb contests Arendt's defense of the "existential supremacy of political action" (p. 7) and her divorce of the pursuit of worldly power and glory from moral constraints. He would exorcise Arendt's heroic and agonistic conception of politics "in order to save our experience" (p. 41).

Kateb decries her linkage of citizenship and participatory politics with the Founders' experience of "public happiness," because that challenges his own celebration of representative democracy.

Kateb fears that any great politics may become vicious, even totalitarian. He affirms representative democracy because it humbles politics, treats all government as "morally dubious, morally inferior" (p. 129). He accuses Arendt of missing the moral authority of representative democracy, of failing to see that limited politics, joined to capitalistic competition, fashions the moral environment in which democratic individuality flourishes. No politicians, but rather Emerson, Thoreau, and Whitman are Kateb's heroes. Last, the consensual foundations of representative democracy legitimize civil disobedience, which, Kateb feels to be, if "not the most heroic [then] as heroic and beautiful as nonmurderous politics can be" (p. 111).

Yet because he substitutes moral discourse for political thinking, Kateb misses much about Arendt and modern democracy. She exposed the political nature of civil disobedience to reveal the power of citizen association that was needed if they were to act effectively in a polity dominated by bureaucratic powers. Kateb admits these vast inequalities, but to save representative democracy bids us act as if it "were completely legitimate, as if its politics were purely the politics of consent" (p. 145). However, even Kateb's democratic heroes, who expressed more contempt for representative democracy than does Arendt, would have rejected such a pretense.

Arendt allowed us access to an American politics greater and more worldly than Kateb's. She reclaimed the republican tradition, with its passion for an active and virtuous citizenship. That tradition, Arendt argued, activated the revolutionary spirit that fashioned the very institutions that Kateb advocates. Arendt felt that for the Constitution to exclude that strong citizenship constituted a real loss. Perhaps, in a representative democracy, civil disobedience is the best politics we can anticipate. But the sometimes politics of an Emerson or a Thoreau pales besides the wordliness of that great American republican, John Quincy Adams. In his diary, he defended a long life devoted to politics:

More than sixty years of incessant, active intercourse with the world has made political movement to me as much a necessary of life as atmospheric air . . . and thus, while a remnant of physical power is left me to write and speak, the world will retire from me before I shall retire from the world. (*Memoirs of John Quincy Adams*, Charles Francis Adams [ed.], J.B. Lipencott, 1874, vol. 10, p. 415)

Kateb attempts to exorcise this sensibility. Arendt recalls the forgotten worldliness of American politics and helps us to recover our public lives and political souls.

MATTHEW F. STOLZ

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Liberalism Reconsidered. Edited by Douglas MacLean and Claudia Mills. (Totowa, N.J.: Rowman & Allanheld, 1983. Pp. xvi + 144. \$32.50, cloth; \$16.95, paper.)

Although there may be spirited debate over precisely what liberalism is and should be, it is relatively well accepted that liberalism is a doctrine under attack and hence on the defensive. The siege mentality of the 1960s may have passed, but the fundamental questions about liberalism's mission and future remain. Indeed, there was a time when liberalism felt it would inherit the earth, but its global status is now one of retreat. And it is by no means obvious that that retreat will be reversed in the near future. However, the necessary, if not sufficient, prerequisite for that reversal is self-clarity on the part of those committed to liberal values, and the present volume represents a serious and provocative exchange that will help foster that clarity.

Some time ago, Alexis de Tocqueville recognized the inherent tension that exists between the liberal commitments to both liberty and equality. This volume is a lively testament to the fact that that tension remains. All of the nine essays confront, and in varying ways try to resolve or overcome, that tension. As it evolves in these essays, the debate is one between proponents of a neutral government that protects the private rights of individuals on the one hand, and proponents of a government that actively intervenes to create at least minimum levels of economic and social equality through redistributive economic programs on the other.

Several of the authors argue that the tension between these competing strands of liberalism can neither be resolved nor indefinitely maintained. Walter Berns in "Taking Rights Frivolously" argues that liberalism should stress liberty understood as the pursuit of happiness privately defined. The end of government is merely to promote the environment within which diverse private ends can be pursued. Theda Skocpol in "The Legacies of New Deal Liberalism" and Christopher Lasch in "Liberalism in Retreat" argue in favor of jettisoning the liberal commitment to individualism in favor of promoting the grounds for a more cohesive community, a large component of which would be to foster greater

social equality that would make a more democratic community possible. Of course, this latter approach raises the spectre of a significant encroachment on individual liberty, and indeed potentially of majority tyranny, an issue Amy Gutman addresses in "How Liberal Is Democracy?" by reflecting on the relation between liberalism and democratic government.

In varying ways the remaining authors try to show that the commitment to both liberty and equality need not cause an irreconcilable tension and thereby the necessary choice between one side of the tension or the other. Ronald Dworkin in "Neutrality, Equality and Liberalism" argues that commitments to both governmental neutrality as regards private individual behavior and to the perception of a responsibility to reduce economic inequality can both be traced to a more fundamental liberal principle. Hence, the seeming tension dissolves. Through a reading of Locke, Nathan Tarcov in "A 'Non-Lockean' Locke and the Character of Liberalism" tries to show that both the commitment to liberty and property rights and recognition of the need to foster community can be found in Locke, the most frequently cited of all liberal authors. And in different ways Marshall Cohen in "Toward a Liberal Foreign Policy," Mark Sagoff in "Liberalism and the Law," and Michael Williams in "Liberalism and Two Conceptions of the State" also try to take off the sharpest edges of the conflict between commitment to neutral government and reformist aspirations.

Throughout, the essays are cogent and relevant, and in a way that is uncharacteristic of volumes of this kind, the authors address each other directly and at times specifically. Hence, this volume will be valuable not only for the much needed discussion of the nature and meaning of contemporary liberalism it will foster, but also as a useful classroom and teaching tool.

GREGORY B. SMITH

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The Sexual Politics of Jean-Jacques Rousseau.

By Joel Schwartz. (Chicago: University of Chicago Press, 1984. Pp. xi + 196. \$17.00.)

Of the many and varied aspects of Rousseau's thought, few are as controversial and seemingly repugnant as his views on human sexuality, women, and the family. In recent years, Rousseau has been almost universally assailed by feminists who see in most of his works, particularly in *Emile*, the musings of a pathological misogynist. In this outstanding book, Schwartz takes issue with this preemptory rejection of Rousseau and

offers a probing and important reinterpretation of Rousseau's sexual politics.

Although Schwartz is clearly a fan, it is not his purpose in this work to launder or make palatable Rousseau's views on women. Indeed, at times Schwartz is almost at a loss to explain the divergent and paradoxical positions Rousseau takes on women and sexuality at various points in his writings. Rousseau, Schwartz argues, views human sexuality as the "bridge to politics" (p. 28), but understanding how that bridge is crossed is complicated by the fact that Rousseau holds two distinct and contradictory teachings about sexuality: "one teaching celebrates sexuality, the other condemns it" (p. 1).

Schwartz argues convincingly that Rousseau views sexuality as more fundamental a foundation of civil society than even agriculture or economics, notwithstanding Rousseau's statement in the *Social Contract* that the "true founder of civil society was the first person who fenced off a plot of ground." Thus, Rousseau holds in his first view of sexuality, as identified by Schwartz, that the mutual dependence bred by sexual relations is beneficial in providing the social impetus. However, because Rousseau always views dependence on other people as the bane of human freedom, he must also resent the development of the family as a consequence of female sexual manipulation because it takes away the possibility of true individual liberty. This is the second view of sexuality in Rousseau that Schwartz identifies, and it is the basis of criticism of Rousseau by feminists from Mary Wollstonecraft to Susan Okin.

Most of Schwartz's book is an attempt to reconcile these two opposed views of human sexuality and women's use of it that Rousseau sees as basic to the political history of the species. This reconciliation is perhaps impossible to achieve other than to argue, as Schwartz does, that for Rousseau both the family and the state are artificial, as opposed to natural, human constructs. Such a conclusion is no less satisfying than Rousseau's fundamental political idea that politics is unnatural but nevertheless capable of bringing much virtue and excellence to human life. The same is true of sexual relations, although like politics generally, such relations are also capable of becoming the chains of modernity that strip modern man—and woman—of their natural freedom.

Perhaps the most important lesson of Schwartz's book is that Rousseau's ideas concerning sex and women are a maze that must be entered and traversed successfully if we are ever to understand his larger political theory. In his chapters on the natural and political history of sexuality, and Rousseau's deep-seated ambiva-

lence to this facet of human beings, Schwartz points the way to a fuller realization of Rousseau's political thought. Although in his conclusion he mirrors his subject's ambivalence on the score of sexual differentiation versus androgyny, Schwartz nevertheless contributes greatly to our understanding of Rousseau's (and our own) search for individual freedom and the barriers raised in that search by the inequities of sexual politics.

RICHARD P. HISKES

University of Connecticut

The Politics of Locke's Philosophy: A Social Study of "An Essay Concerning Human Understanding." By Neal Wood. (Berkeley: University of California Press, 1983. Pp. xiv + 241. \$26.50.)

There are two books wrestling for control between the covers of this little volume. The first is an unexceptionable and sometimes insightful account of the social content of Locke's philosophy. The second is a problematic argument that Locke's views are illuminated by the imposition of Marxist categories. Wood is of two minds throughout; I suspect he never finally decided what he wanted to say, and the resulting text is contradictory, not dialectical.

Wood wants to rescue the *Essay* from today's analytic philosophers. He is willing to concede that it contains technical discussions of rarefied epistemological problems, but he wants to focus on the social ramifications of Locke's theories. The empiricist project for pursuing knowledge summons up a view of an individual critically evaluating the evidence, refusing to knuckle under to Aristotelian schoolmen or noble lords. Locke is fully aware, more I think than Wood acknowledges, that the accumulation of knowledge is a social process; for all the brilliance of "the incomparable Mr. Newton," we need a society where people can weigh competing evidence freely.

That means we need to dispense with the nonsensical maxims of the schools, with heated devotion to religious sects, and with mindless repetition of past verities. Wood suggests, I think quite rightly, that Locke is following up Bacon's attack on the idols of the mind. Again rightly, Wood

sees that Locke's chief concern is religious faction. Puritan and Catholic fanatics not only make for religious civil war; they also cram their followers' heads full of nonsense, preventing them from pursuing the truth.

So far, so good. But sometimes Wood wants to cram the richly varied dimensions of Locke's social concerns into one category and unmask Locke as the teacher of the bourgeoisie. After discussing science, religion, politics, the changing reading public, and more, Wood concludes, "The *Essay*, therefore, both in England and America, had a definite appeal for the emergent bourgeoisie" (p. 64). After correctly arguing that the parties that so exercise Locke are chiefly religious sects, Wood suddenly announces that Locke perhaps opposed the Puritan Revolution because of "its momentary threat to the propertied and promise of fundamental change to the common people" (p. 110). A cognitive ego out to accumulate true beliefs is, we are told, just like a conative ego out to accumulate pleasure for itself, and that's just like capital accumulation (p. 160). Locke's attack on scholastic jargon and pettifoggery is "by implication" a rejection of natural inequality (p. 128).

This simply will not do. Here Wood deliberately and perversely collapses his thesis; Locke's concerns were surely much broader. Only a flat misreading of the *Education* will make Locke a believer in the unalterability of self-interest. And although Wood uses it freely, *bourgeois* has become all too flexible a concept, more a term of abuse than a precise category.

Sometimes (pp. 118, 126, 178, 180) Wood distinguishes between finding Locke's intentions and charting the likely effects of the *Essay*. Here, if he likes, Wood is surely free to speculate on economic class, but his view is still murky because his Marxism is muddled. Wood wants to "eschew a dogmatic and mechanical approach," but he insists that "class is a fundamental lever of social change" (p. 13). He thinks J.H. Hexter and Lawrence Stone dismiss class because "they mistakenly conceive of class as an entity or structure instead of a relationship" (p. 12), but he himself characterizes class as an entity (p. 13). Marxism here, unhappily, is an idol of the theater that interferes with an otherwise fine study.

DON HERZOG

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Empirical Theory and Methodology

Strategic Planning and Forecasting: Political Risk and Economic Opportunity. By William Ascher and William H. Overholt. (New York: John Wiley & Sons, 1983. Pp. xii + 311. \$29.95.)

According to the authors, political forecasting attempts to anticipate three classes of trends or events. First, there is the distribution of power among institutions and individuals. A second focus is the climate of politics that encompasses how specific political events are played out. Finally, political forecasting examines the continuities and changes in authoritative policy and in actor strategies. Ascher and Overholt seek to provide both an overview of the field of political forecasting and to reorient our thinking about the subject, moving the discussion away from the relative merits of competing mathematical models to a study of political forecasting in the context of the policy process. The authors contend that it is only in this way that forecasting can regain its credibility and become useful to policymakers.

Ascher and Overholt succeed admirably on the first point. They provide a highly readable and thorough overview of forecasting methodology, the logic of political forecasting, and the relationship of forecasting to planning. Central to the authors' review of political forecasting is the assertion that the principal task of the forecaster is to describe the future environment in terms that are useful to the policymaker. This is best done by viewing forecasting as a heuristic device that has its greatest impact on the policymaker through the presentation of scenarios. The construction of scenarios is urged over the employment of complex models because of the need for comprehensiveness in many forecasting efforts and for the ability of scenarios to project the dynamics of politics rather than just project a trend or predict an event. The most effective analytical device for structuring scenarios is held to be viewing politics as organizational activity.

Ascher and Overholt are less successful in meeting their second objective, that of anchoring political forecasting in the context of the policy process. Their treatment of this subject tends to be disappointing. It does not take us beyond our present understanding of the difficulties of integrating forecasting, planning, and policy. Moreover, issues discussed at length in the literature on intelligence estimating and surprise (areas certainly encompassed by political forecasting) are

left largely untouched. For example, the authors note the difference between long-range and short-range planning, but no parallel discussion exists regarding the distinction between short- and long-range forecasting. This omission is significant because of the differing problems encountered in integrating the two into the policy process. Long-range forecasting faces neglect, while short-term forecasting faces politicalization.

To some extent the problems encountered here stem from the structure of the book. In its method of presentation, *Strategic Planning and Forecasting* is typical of many current business-oriented texts. Frequent section headings and subdivisions are employed to cover the subject matter. The result is a tendency to highlight the central features of a problem rather than to explore them in an in-depth and integrated fashion. This point can be illustrated through the use of case studies. Ascher and Overholt employ three well-written case studies on political development to argue in support of a heuristic and scenario centered forecasting methodology. They also begin their book by introducing a thoughtful model linking forecasting and strategic planning into a coherent whole, yet, none of the case studies constitutes an effort to demonstrate the practical utility of this approach. They are instead all written as self-contained essays.

In sum, Ascher and Overholt have provided us with a valuable overview of political forecasting and pointed us in the direction of more thoroughly linking forecasting to policymaking. The nature of those linkages, however, still eludes us.

GLENN P. HASTEDT

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The Evolution of Cooperation. By Robert Axelrod. (New York: Basic Books, 1984. Pp. x + 241. \$17.95.)

Robert Axelrod's *The Evolution of Cooperation* is a unique effort to employ the prisoner's dilemma in exploring the possibilities of extending and improving the limited cooperative proclivities of humans. Axelrod is fascinated and possibly mesmerized by the analytical utility of game theory in general, and the prisoner's dilemma in particular. Fascination with games of mixed motives as the primary means of gaining leverage

on promoting cooperation is deserving of study in and of itself. For scholars appreciative of harmony, cooperation, and peace, the Hobbesian state of nature or zero-sum model possesses an irresistible appeal and immediacy; the more sanguine and realistic non-zero-sum model of Locke holds less interest. Perhaps, the intellectual challenge of meeting the Hobbesian authoritarian solution is a more demanding task. Axelrod seems to vacillate between the two models, using the zero-sum game to dramatize the imperativeness of cooperation and non-zero games as the means by which we can resolve our differences. The problem is twofold: Everyone chooses the more profitable defect strategy, and/or most people fail to see the gains of cooperation.

Axelrod's inquiry proceeds with an interesting combination of historical cases including trench warfare in World War II, a study of biological evolution, game theory proper, and a fascinating computer tournament involving many well-known students of game theory. From these sources, Axelrod discovers that the best strategy for participants in prisoner's dilemmas is pursuit of a "tit for tat" strategy, that is, the player cooperates on his first move and then does whatever the other player did on the previous move. Axelrod then offers advice to participants and reformers in Part 4 and Part 5 on how to promote cooperation in mixed motive situations. This is fascinating material, especially for social psychologists and game theorists. Axelrod claims that cooperation can be initiated and encouraged by enlarging the shadow of the future, changing the payoffs, teaching people to care, teaching reciprocity, and improving our recognition abilities of mutual gains—advice proffered by most authors of books on how to win friends and influence people.

I find it odd that Axelrod and his fellow game theorists seldom inquire into the market economy as a means of cooperation; indeed, one can make a powerful case that the market economy is the most robust institution for cooperation ever to have evolved. If this is the case, then a history of market economies done in terms of the prisoner's dilemma might shed not only a great deal of light on the evolution of markets, but it also might add to our capacity to promote cooperation. How did markets emerge from the prisoner's dilemma of ancient times? How do markets enable participants to overcome the defect option?

Although Axelrod does not list markets in the Index, the text does refer to a handful of market examples including cartels; interestingly, all deal with market failure, that is, imperfect markets. (Almost all applications of game theory in economics are to oligopoly!) And, Axelrod correctly notes, these are instances of "bad" cooperation, because they reduce the supply of

goods and increase consumer prices for non-members of the cooperative group. Trade wars are also briefly mentioned, but they are really examples of inefficient public policies adopted as much for the advancement of the interests of politicians as for inefficient home industries.

But if normal cooperation is viewed as a good thing, why do game theorists not buttress their case with citations from the history of free-market competition? That markets promote cooperation has been a powerful theme in the writings of all its advocates from Adam Smith to von Hayek. Markets, it would seem, go unappreciated because of the transparency of self-interest and the nonobvious workings of the hidden hand of exchange. Accordingly, markets appear as divisive, exploitative, manipulative, and uncertain—the opposite of conscious cooperation. Still, Axelrod's advice on promoting cooperation would seem best exemplified in everyday exchange activities of the marketplace. Surely the market is the best example we have of a non-zero-sum game. Politics, on the other hand, converts the latter game into zero-sum situations.

Despite my stricture, Axelrod's thoughtful and lucid book is a worthy addition to the literature of game theory and, indeed, a worthy addition to political theory. It deserves the widespread favorable attention it has already received.

WILLIAM C. MITCHELL

University of Oregon

The Spatial Theory of Voting: An Introduction.

By James N. Enelow and Melvin J. Hinich.
(New York: Cambridge University Press, 1984.
Pp. xv + 238. \$39.50.)

The title of this book indicates it will introduce the uninitiated to the structure of spatial theories of voting. This notion is reinforced by the authors' claim to bring the reader "step by step" to "a full appreciation of the contributions spatial theory has made to our understanding of voting" (p. 1). The incremental approach breaks down, however, after the third chapter of this 10-chapter book. Neither does this work, like many others in positive or formal political theory, live up to its promise that "the level of mathematics needed to express the important results in this field is surprisingly modest" (p. xiv). Too often claims to this effect are made when they are unwarranted, usually accompanied by the assertion that the book is written for the general reader. Such promises are misleading when the authors consider, as in the case of the book under review, a "modest" mathematical background to include geometry, quadratic equations, probability

theory, and calculus. The reason I dwell on this point is that positive or formal theory has penetrated virtually every subdiscipline of political science. Thus, every student of politics can benefit from the non-obvious conclusions that characterize this rapidly burgeoning literature. But many are intimidated by its mathematical nature. Therefore, is it extremely important that a clear and accurate statement describe for potential readers when a particular work is an introductory text, a nontechnical discussion that uses the spinoff from the literature, or a technical discussion for specialists in the field. Enelow and Hinich's book falls into the latter category.

The major objective of this book is to develop a new spatial theory of voting, one that can explain the dynamics underlying both committee voting and voting in mass elections. Thus, the core of the book is chapter 4, where the authors develop their new model. Here Enelow and Hinich successfully "rebuild the multidimensional theory of candidate competition in mass elections" (p. 79). This is accomplished through the skillful unfolding of the relationship that exists between the classic right-left ideological predictive dimension voters use to label candidates with the perceptual views of individual voters. Then the link between policy issues and the ideological predictive dimension is demonstrated. In this fashion, Enelow and Hinich manage, with methodological agility, to lay the foundation for a broad theory of party competition through the use of a spatial model of candidate competition that explains how "voters estimate the issue positions of each candidate by translating the candidate's position on an underlying predictive dimension into a set of positions on the policy issues of the campaign" (p. 54). Chapter 5 incorporates nonpolicy issues or the fixed issues of a campaign such as gender, religion, peace, and prosperity into the model. In chapter 6, the model is generalized to encompass committee voting. Chapter 7 demonstrates how the model can be applied under conditions of candidate-induced uncertainty, the perceptual uncertainty of voters about the point along the ideological predictive dimension where candidates ought to be located, and predictive uncertainty about what candidates will do when elected. Chapter 8 provides an interesting discussion about the impact institutions can have on voting outcomes. And chapter 9 provides empirical verification of the model through an analysis of the 1976 and 1980 presidential elections.

Although I cannot recommend this book as an introduction to spatial theory, it does have something of value to specialists in the field.

THOMAS J. MORTILLARO

Nicholls State University

Social Impact Assessment Methods. Edited by Kurt Finsterbusch, Lynn G. Llewellyn, and C.P. Wolf. (Beverly Hills, Calif.: Sage Publications, 1983. Pp. 318. \$29.95.)

This book offers an inventory of social impact assessment (SIA) methods divided into four parts: a paradigm of SIA, primary data collection methods, secondary data collection techniques, and special problems and methods. In Part 1, Wolf presents a rational problem-solving scheme of "anticipatory research" for assessing potential winners and losers and providing "use knowledge" to mitigate and manage "people impacts." Michael J. Carley discusses a number of numerically oriented methods and argues for more extensive preprogram assessments. Cynthia Flynn et al. present a group ecology method for assessing social and economic change linking the direct effects of a project to the probable social and economic responses of the people affected.

Part 2 begins with a thorough cataloguing by Finsterbusch of the multiple SIA uses of survey research. Roy Roper discusses the role of ethnography, focusing on the effects on both individuals and families of potential sociocultural disruptions. Annabelle Bender Motz explores in Part 3 the virtues and drawbacks of historical documents, the use of which is often limited by their questionable validity and reliability. Thomas Dietz and C. Mark Dunning examine demographic impact assessment methods, with emphasis on comparing a baseline "without project" scenario against "with project" scenarios for assessing the impact of policy-induced demographic changes on community infrastructure and local service demands. Demographic simulation models are also presented. Carley discusses the surprisingly limited overlap between SIA and social indicators research.

Part 4 addresses six special topics. Steve H. Murdock and F. Larry Leistritz evaluate the informational characteristics, methodologies, and applicability of 13 widely used socioeconomic impact assessment models. Rabel J. Burdge elucidates the role of direct public input from key informants, advisory groups, community forums, nominal group exercises, Delphi techniques, questionnaires, and jury panels, and the use of secondary information and agency records. John W. Lounsbury, Kent D. Van Liere, and Gregory J. Meissen provide a psychosocial "attitude theory" framework for SIA. Jeryl Mumpower and Barry F. Anderson discuss the twin problems of random judgmental inconsistency and systematic judgmental bias. Tentative solutions to both problems are explored, notably the triangulation of methods for converging toward correct solutions. James F. Palmer highlights the visual aspects of

impact assessment. Finsterbusch concludes by emphasizing the ethical dimensions of SIA and discussing techniques for multiobjective decision making in a somewhat abstracted presentation of utilitarian democratic theory and principles of ethical pluralism. He argues for three assessment cycles in SIA work: eliminating unacceptable alternatives, eliminating those that are unpromising, and selecting the best remaining option. As with much else in the SIA literature, this is much easier to put into print than it is to implement.

This is not a statistically oriented book, unlike others of the evaluation genre, and the discussion is often overly general. This is not to say that the content is unrealistic, only that it sometimes lacks specificity. One particular difficulty is the diverse nature of the methods that are presented. Eclecticism is a two-edged sword, producing a lack of focus as well as a wealth of alternative approaches. SIA is therefore difficult to define, except in specific operational contexts. One source of this lack of clarity is the often deliberately ambiguous wording and consequent latitude for interpretation in national legislation. What should be criticized for lack of definitiveness perhaps is not the methods of impact assessment, but rather the political process that creates the problems that must be addressed by those methods.

MACK C. SHELLEY, II

Iowa State University

Contemporary Public Policy Analysis. By Stuart S. Nagel. (University: University of Alabama Press, 1984. Pp. xiv + 174. \$17.75, cloth; \$7.50, paper.)

Stuart Nagel's latest book on public policy analysis is taken from a series of lectures given in 1982 at the University of Alabama. The lectures and the book try to do two things: provide an introduction to the field of public policy analysis, and discuss benefit cost analysis as a specific technique in policy analysis. The book succeeds in the introductory aspects, but, unfortunately, fails in the analysis portion.

Taking the successful portion of the book first, Nagel presents a comprehensive list of institutions, periodicals, and approaches to the study of policy analysis. He notes, for example, the Kennedy School's emphasis on economics, Berkeley's emphasis on political science, and Northwestern's social-psychological approach to policy analysis. He presents good descriptive material on journals and publishers in policy analysis in his chapter,

"Trends in Policy Analysis." There also is a brief but interesting description of the differences between business analysis and analysis for the public sector. The concluding chapter on ethics is informative, and throughout the book interesting examples are provided from the area of Nagel's expertise, criminal justice. But even within these relatively good sections of the book, there is excessive "list-making," which is annoying to the reader and distracting to the flow of the writing.

The bulk of this book is a rather tedious and drawn out application of benefit cost techniques to various problems in policy analysis. In fact, the book might be more accurately retitled "Benefit Cost Analysis in Contemporary Policy Studies." The analysis would be useful for readers who are interested in reviewing basic algebraic principles and applying them to situations with known and unknown variables, monetary and nonmonetary benefits, mutually exclusive and combinations of projects, and all permutations thereof. But most readers are not interested in such a review and could work out the applications themselves with some basic knowledge of algebra.

Furthermore, there is nothing new or exciting about the discussion of scholarship in the area of benefit cost analysis. Nagel points out that using the formula benefits minus costs ($B - C$) is superior to dividing benefits by costs (B/C) in formal analysis. But this is nothing new; economists have been using this formula for the past 20 years when comparing multiple projects. The only readers who might find this information interesting are government agency analysts who have not kept up with the literature on benefit cost analysis.

In sum, Nagel's latest book is a disappointment.

JAMES S. LARSON

University of Arkansas at Little Rock

The Parable of the Tribes: The Problem of Power in Social Evolution. By Andrew Bard Schmockler. (Berkeley: University of California Press, 1984. Pp. xi + 400. \$19.95.)

This wide-ranging, cross-disciplinary book is frustrating to review. On the one hand, it deals with significant issues—the evolution of civilization and the state, the causes of war, the roots of alienation. On the other hand, it is often a collection of commonplaces from various disciplines surveyed and an oversimplification or inaccuracy in exposition.

The basic organizing concept of the book is, as the title goes, "the parable of the tribes." This

parable is based upon the following situation (p. 21):

Imagine a group of tribes living within reach of one another. If all choose the way of peace, then all may live in peace. But what if all but one choose peace, and that one is ambitious for expansion and conquest?

The end result is that the tribes compete for power—"A selection for power among civilized societies is inevitable" (p. 22). In one sense, the basic argument underlying the book is little more than a modest variation on the security dilemma, or, more classically, Rousseau's stag hunt metaphor (see Kenneth Waltz, *Man, the State and War*, Columbia University Press, 1959).

Schmookler argues that although human biology and psychology shape human behavior considerably, social and cultural norms may override these, and consequently produce the circumstances in which humans come to live in a civilization that is not consistent with their nature. The result? Alienation. However, Schmookler's discussion is partially undermined by some rather questionable assumptions. For example, at one point he embraces the concept of "group selection" to explain the evolution of human altruism, an explanation not acceptable to most contemporary evolutionary theorists. His "conquest theory" of the evolution of civilization and the state is in the mainstream of anthropology as far as I can tell, but the arguments go little beyond what Marvin Harris and John Pfeiffer, among others, say.

An important concept for Schmookler is "synergy." He defines a synergistic interaction as "one in which each part functions in a way that enhances the welfare of the other parts as well as its own" (p. 216). This concept is part of a plea by Schmookler for humans to realize the necessity of understanding their place in nature. His actual use of synergy, though, does not seem to depart too much from a standard systems viewpoint. His analysis does not examine the issue in nearly as detailed a fashion as Peter Corning's *The Synergism Hypothesis* (McGraw-Hill, 1983).

Later in the book, Schmookler offers critiques of both capitalism/liberalism and Marxism. One difficulty is his lack of awareness of refinements in both ideological camps. His attack on Marx is a full-scale assault on a straw man. Twentieth-century Marxists such as Gramsci and Habermas simply have to be considered given Schmookler's battle plan against Marxism.

Schmookler's lack of expertise may be most evident in his concluding chapter—a mere six pages, which one would think to be the culmination of the argument. He concludes that humans must better integrate their nature (rooted in their

hunting-gathering past) with the demands of advanced civilization. This is surely anticlimactic; worse, there are no hints as to how this is to take place given the dynamic of "the parable."

All of these problems notwithstanding, this is an interesting book. Schmookler dares to attempt a grand synthesis; his failure to achieve his goals is thereby rendered less significant. His ambitious effort may make this book pleasant reading.

STEVEN A. PETERSON

Alfred University

Genius, Creativity, and Leadership: Historiometric Inquiries. By Dean Keith Simonton. (Cambridge, Mass.: Harvard University Press, 1985. Pp. ix + 231. \$20.00.)

This book sets out to bring together, evaluate, and to suggest the significance of published material—principally but not exclusively quantitative—dealing with the factors that appear to be associated with artistic and scientific creativity, and with remarkable political leadership. Simonton has himself in the past decade or so published some of the best work in this area.

Factors such as the effects of heredity, intelligence, goodness or badness of character, personality type, level and type of education, productivity, age, *zeitgeist*, and political turmoil are all evaluated. The problems that inevitably arise concerning historical accuracy, historical comparability, and conceptual clarity are presented carefully.

Simonton's discussion of family influences—birth order, orphanhood, and family heritage—is exemplary. Francis Galton's, Havelock Ellis's, and other studies indicate, for example, that first-born children are more apt to be major scientists or artists, whereas political leaders are much more likely to be found among middle children. The question of only-child status, and of revolutionary as opposed to integrative leadership, are also considered, as is the effect of preferential educational treatment for eldest children. Simonton suggests that revolutionary leadership is more associated with living in a large family (which of course increases the chance of being other than an eldest child) than of being a middle child per se.

One melancholy conclusion that parents ambitious for their children's success should note is that the very best thing they can do to help their children, regardless of birth order, is to die, the sooner the better: "the proportion of orphanhood among . . . geniuses far exceeds those found in comparison groups" (p. 29). It is not strictly necessary to die; the loss can also be by events such as divorce or alcoholism. As Simonton

notes, however, it is the death of a same sex parent that has the major effect, and, most of the instances he discusses being male, this event may lead to an even more important influence: "Poets, dictators, and military heroes have one biographical datum in common: they exhibit a high percentage of dominating, possessive or smothering mothers" (p. 29).

Parenthetically, in his section on famous families, Simonton argues that when intelligence and education (both family-linked) are controlled, family itself has virtually no independent explanatory power.

In such ways this book seeks to use the data of history and the methods of the social sciences to move toward generalizations about the social determinants of creativity. More important perhaps than the tentative (although very useful) generalizations that Simonton offers, he shows that systematic inquiry can produce firm and testable generalizations of good explanatory and predictive utility.

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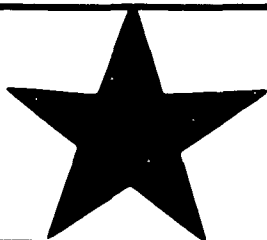
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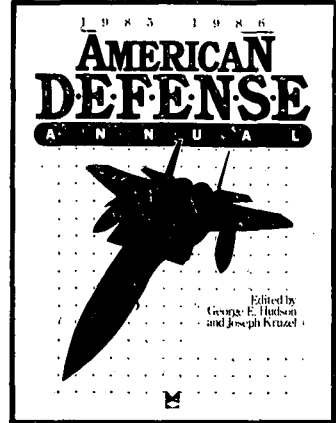
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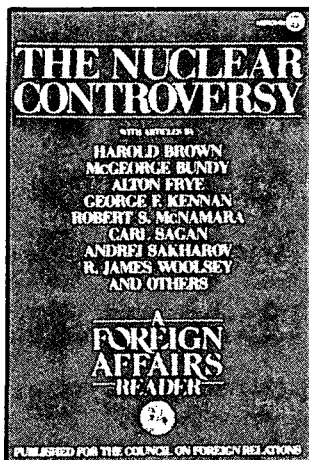
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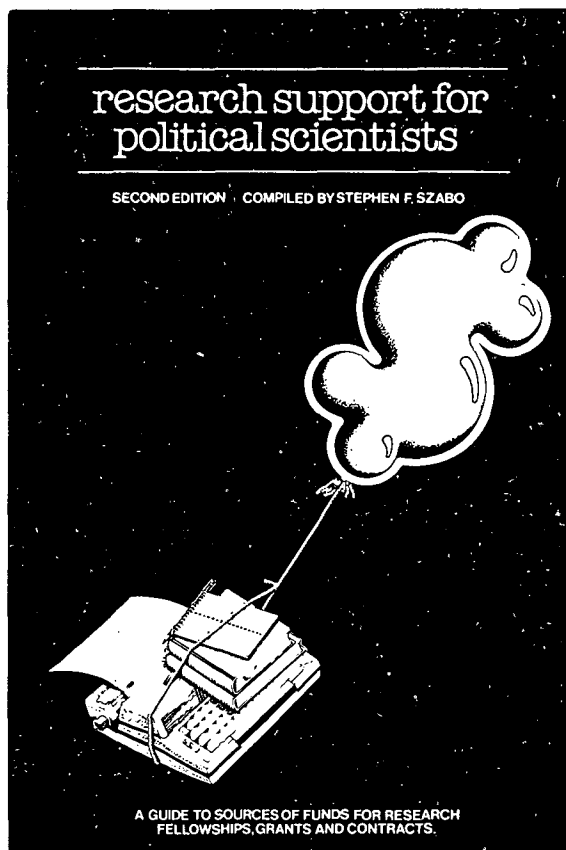
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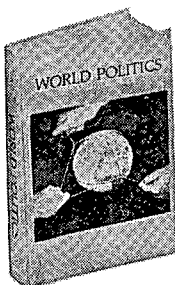
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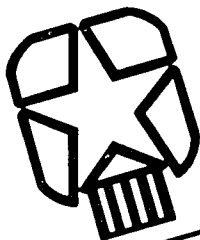
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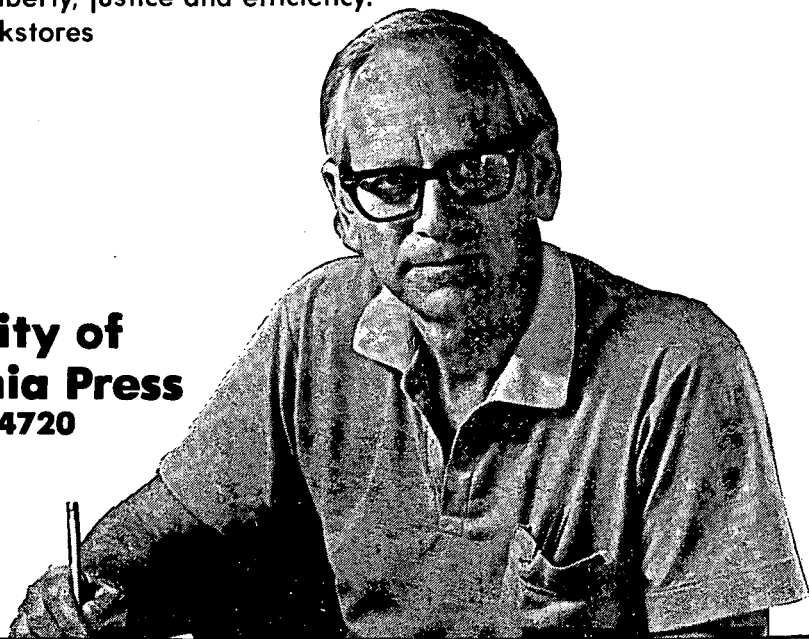
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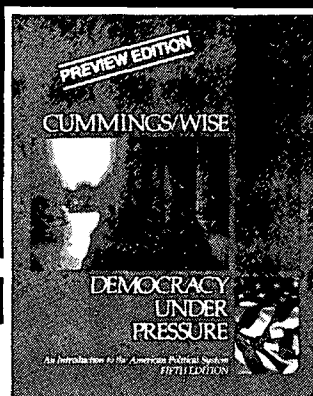
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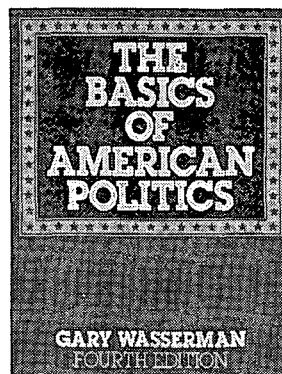
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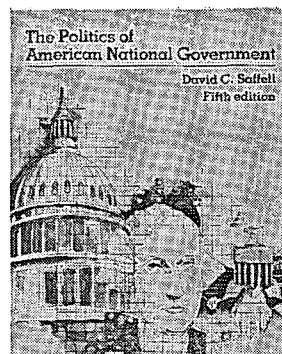


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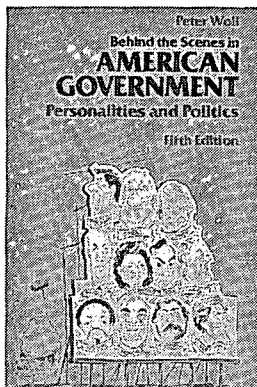
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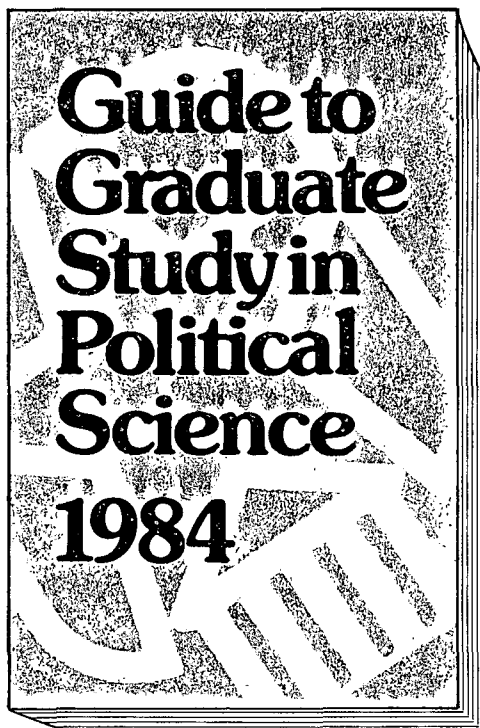
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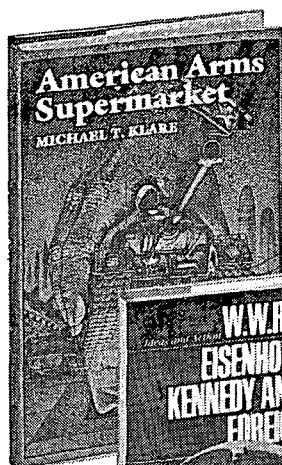
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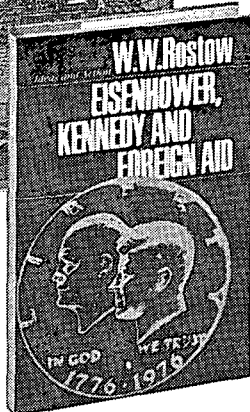
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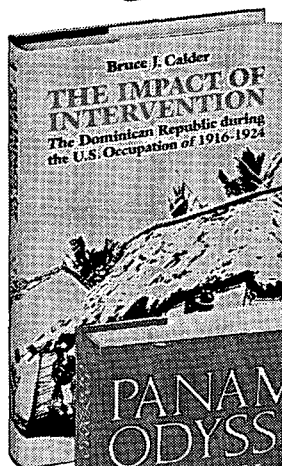
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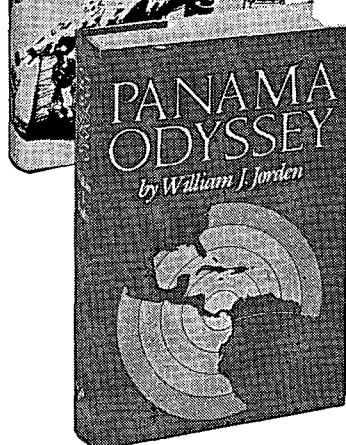
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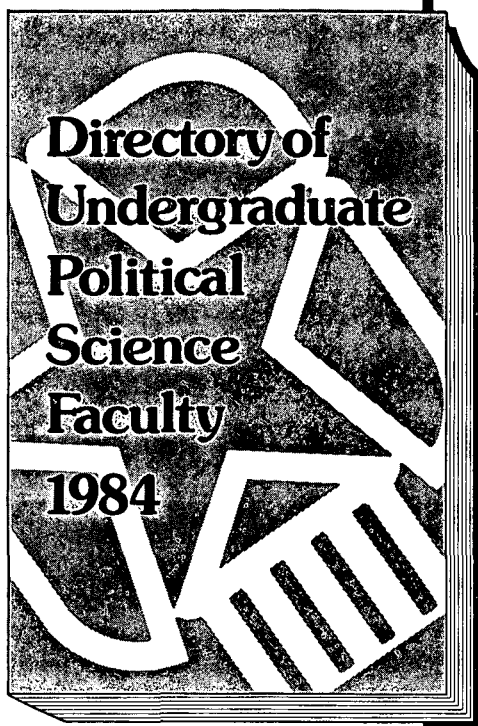
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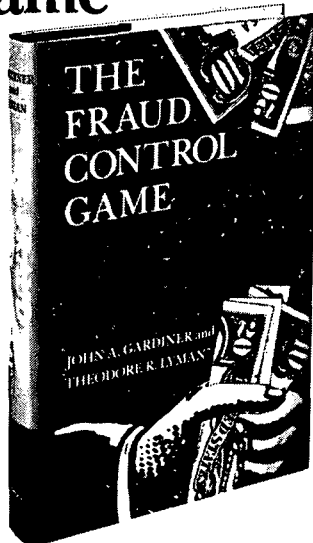
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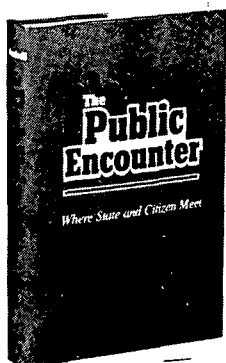
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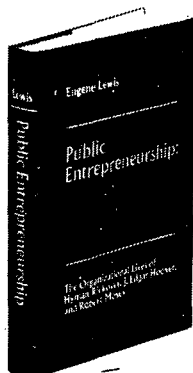


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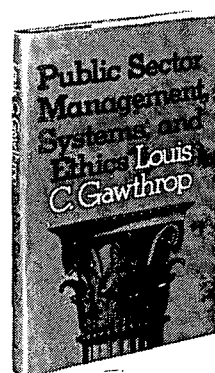
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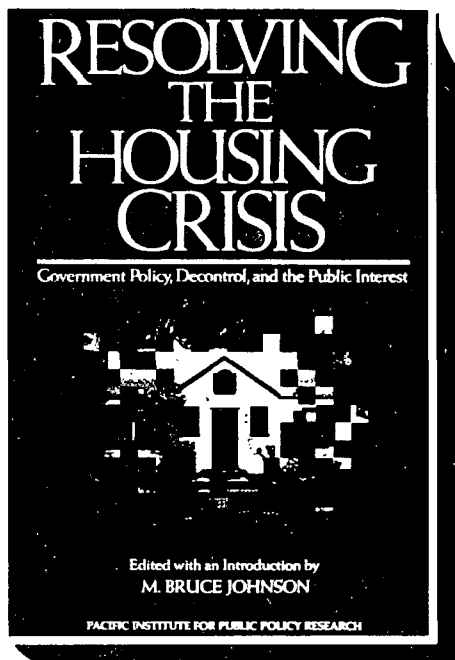
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